# QUARTERLY JOURNAL
OF THE
# ROYAL METEOROLOGICAL SOCIETY

*Editor*: T. W. WORMELL

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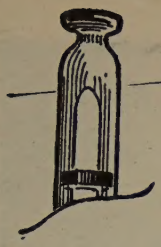

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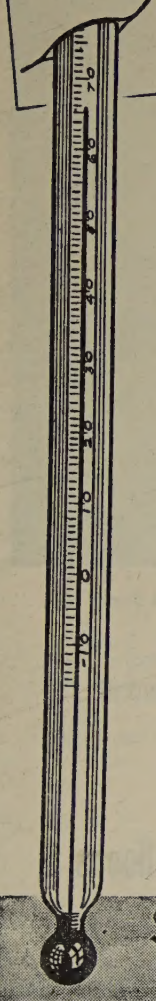

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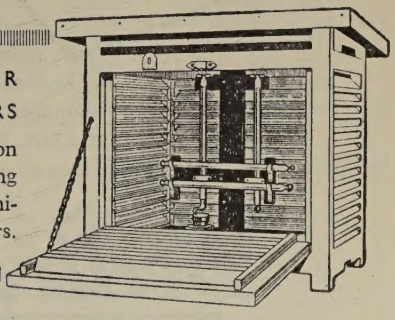

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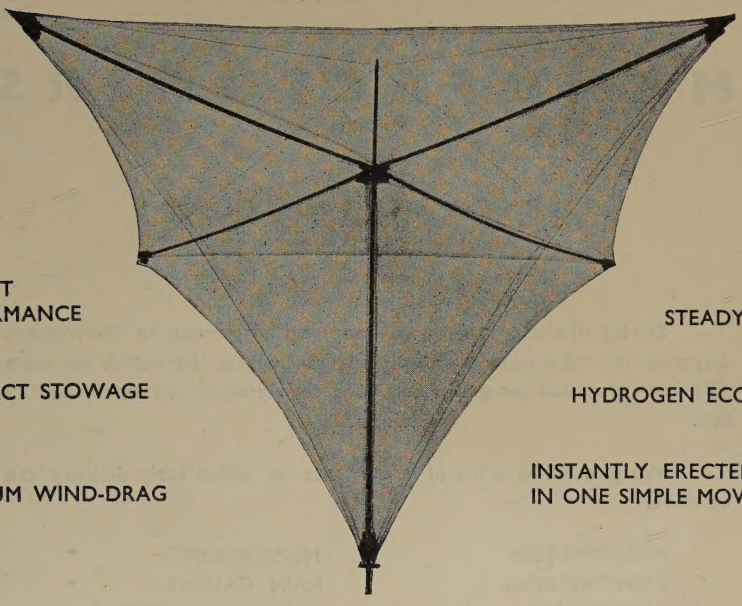

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# Monsoon seasonal forecasting

By SIR CHARLES NORMAND, C.I.E., D.Sc.

Presidential Address delivered before Royal Meteorological Society on 17 June 1953

As seasonal forecasting has been attempted over a period of seventy years in India, this is a suitable opportunity to select what seem to me the salient points in that history and to summarize some of the results of experience. I do not claim to speak as a research worker in the subject, but rather as a close onlooker and practitioner.

The seasons, rather than the daily weather, are the dominant features of Indian meteorology. The course of the monsoon is a vital matter to agriculture; and a great mass of the population depend directly upon agriculture for their daily bread. While industry and trade certainly play a larger part in the economy than they did, failures of the monsoon rains, as in the famine years of 1877, 1899 and 1918, would still embarrass the budget of the Central Government and thoroughly upset the budgets of the Provincial Governments in the famine areas. A Finance Minister once declared that his budget was a gamble in rain.

The need to attempt to forecast or foreshadow each monsoon is patent to all, and it is natural that the meteorologist should be called upon to make the attempt, although a few orthodox members of Indian society may still ask – ' but who better or cheaper than the astrologer ? ' H. F. Blanford was called upon to make a tentative monsoon forecast in 1878 after the great 1877 famine, and we can certainly understand a Madras Famine Commission recommending that ' So far as it may become possible, with the advance of knowledge, to form a forecast of the future, such aids should be made use of, though with due caution.' That was written in 1881. From 1882 to 1885, H. F. Blanford prepared further tentative forecasts before each summer monsoon, in the last of which – 1885 – the ominous sign of heavy snowfall in the Himalayas was given full weight. The predicted delay in the onset of monsoon rains in south and west India was borne out by events, and some degree of confidence was established, and what one might call the Official Monsoon Forecasts began in 1886. By then H. F. Blanford was nearing the end of his Indian service. He had started as a geologist alongside his equally famous brother (W. T.), but when his health broke down under the strain of prolonged field-work in the jungle, geology's loss was our gain, for he became the Meteorological Reporter for Bengal, and later on (in 1875) started the All-India Meteorological Service, to which he gave an admirable lead.

Early on he had noticed that the great droughts seemed to be preceded by excessive winter or spring snowfalls in the Himalayas and the Hindu-Kush, and it seemed a clue in which he could place some trust as the cold reservoir created by the snowfall was a possible and plausible causal connection. Looking around for other connections, he

examined the pre-monsoon distribution of pressure and temperature in India, and finally (in 1887) anticipated a little of the work of Sir Gilbert Walker, by inferring that droughts may be associated with high pressure in Mauritius, Australia, and over a great part of Asia.

Sir John Eliot followed Blanford. While holding professorial chairs in India he had become interested in meteorology and had written many papers on cyclones of the Bay of Bengal. He had little or no scientific staff and worked strenuously himself to improve and extend the service. He was in his own person a complete meteorological service, and, undoubtedly, weight of work left him insufficient time to weigh the validity of his methods in monsoon forecasting. For example, contrary to the earlier judgment of Blanford and the later statistical finding of Walker, Eliot inferred that an excess in barometric pressure at Mauritius favoured monsoon rains, perhaps under the plausible enough expectation that the higher pressure would drive more air northwards from the South Indian Ocean to feed the monsoon current. It is regrettable that under his direction the monsoon forecasts became more and more ambitious, though based on a slender foundation, and extended to thirty printed foolscap pages, with anticipation in considerable detail. Newspapers became rather scathing in their comments. So about 1901, under the aegis of the central government, Eliot was protected by an order that the forecasts should be issued to the provincial governments as confidential documents.

Let us not forget that, monsoon prediction apart, Eliot wrote much of scientific value, produced a magnificent *Climatological Atlas*, and was a first-class administrator. He longed in vain for scientific assistants for himself, and managed to secure them for his successor, Sir Gilbert Walker, who gave up a Fellowship at Trinity College, Cambridge just fifty years ago to go out to India. It is of interest to recall that Eliot had been Second Wrangler and First Smith's Prizeman, and Walker Senior Wrangler at Cambridge.

Walker faced the fact that he had to examine the possibilities of monsoon forecasting and saw that he had to come to an early decision on three points in particular : to fix a standard of validity in relationships; to settle the degree of detail to be aimed at; and to determine the extent of the search – whether it should be world-wide or limited to the Indian Ocean area.

Eliot had used the methods of analogues and of parallel curves, both of which are subjective and liable to differing interpretations under differing examiners, when the connection is slight and the data are few. Walker swung over to an objective test. He quickly arrived at and used a $\Sigma xy$ criterion, before realizing that the Pearson correlation coefficient was the better indicator. He became a main pioneer in the use of correlation in meteorology – for which meteorologists have not all accorded him whole-hearted thanks. Whatever others may have been, he himself was critical in the use of this tool. He was well aware of its limitations. He saw that the random error of a C.C. (correlation coefficient) does not apply when the few highest of a large number of C.Cs are selected, and he established a criterion to deal with that situation. He would remind his colleagues that linearity of relationship, on which the validity of a C.C. depends, is not a law of nature, and that a crop that prospered in a moderate rainfall might find a deluge as bad as a drought. But in the absence of any better objective test, he determined to use the correlation method.

On the question of detail it was clear enough that one can choose too small an area. It is better to combine several sub-divisions that correlate well, as the total then appears a more meaningful entity. On the other hand India as a whole is too large to be treated as a single unit. Some areas are negatively correlated with others, for example, the monsoon rainfall of Bengal and Assam with Bombay and Central India. Walker finally used the areas NW. India, NE. India and the Peninsula, which despite the name consists of a

TABLE 1. FACTS ABOUT INDIA'S RAINFALL

| Monsoon rainfall | Normal (inches) | Extreme deviations (inches) | Year | Standard deviation (inches) | Random chance 4 to 1 Within (per cent) | Above (per cent) |
|---|---|---|---|---|---|---|
| June-Sept. | | | | | | |
| Peninsula | 34·1 | − 16·6 | 1899 | 5·1 | ± 19 | 87 |
| | | + 10·3 | 1878 | | | |
| NW. India | 19·4 | − 11·6 | 1877 | 4·6 | ± 30 | 80 |
| | | + 14·0 | 1917 | | | |
| NE. India | 52·6 | − 9·9 | 1884 | 4·1 | ± 10 | 93 |
| | | + 8·0 | 1922 | | | |
| Monsoon rainfall | | | | | | |
| Aug.-Sept. | | | | | | |
| Peninsula | 16·9 | − 8·9 | 1899 | 3·9 | ± 30 | 81 |
| | | + 10·8 | 1878 | | | |
| NW. India | 11·0 | − 9·2 | 1877 | 3·8 | ± 44 | 71 |
| | | + 10·4 | 1917 | | | |
| Winter precipitation | | | | | | |
| Jan.-Mar. | | | | | | |
| NW. India | 2·8 | − 2·1 | 1902 | 1·3 | ± 58 | 63 |
| | | + 3·7 | 1911 | | | |

truncated portion of the Indian Peninsula, omitting the southern portions. The facts concerning the seasonal rainfall in these three areas were conveniently summarized by S. K. Banerji in a table which is reproduced here as Table 1.

Especially notable in this table are the large extreme deviations and standard deviations in NW. India and the Peninsula, relative to the normal rainfall, in spite of the extent of these areas.

For the main analysis of the relationships of the summer monsoon with preceding conditions Walker used the total period June to September, not the individual months or half-months or weeks, i.e., he used one number to express the normally variegated pattern of a season's rainfall. Simpson pointed out the considerable degree of independence between the rainfall of the different months – the rainfall of June especially so. If the months have little interdependence, can one legitimately combine the months and yet expect to find the whole has its relationships with other events ? We would expect better forecasts for the separate months, or even for the period July-September, than for the whole period June-September. That, however, has not proved to be so in practice, and it seems to be more difficult to forecast for a portion of a season than for the whole.

To express and summarize all the many incidents in a season's rainfall over a large area by a single figure – by one parameter only – is a drastic procedure. After so much smoothing does anything of real meaning remain ? To the individual farmer the timing as well as the amount of rain is important. After the event, indeed, the agricultural meteorologist prepares rainfall summaries week by week and region by region, to study the weather's contribution to the crop yield, and one can choose many illustrations out of the diagrams prepared in India by Dr. L. A. Ramdas and his colleagues. Fig. 1 shows the regional weekly rainfall in the years 1917 and 1918, contrasting a flood year and a famine year. The dotted curves show the normal, and the notched areas the actual. The contrast between the two years is great in the United Provinces (U.P.) and NW. India and not noticeable in NE. India. In the Peninsula the rains arrived much too early in 1918, and faded away when they should have been most plentiful. Diagrams of this sort before the event would constitute the ideal seasonal forecast, and are doubtless what Eliot had in

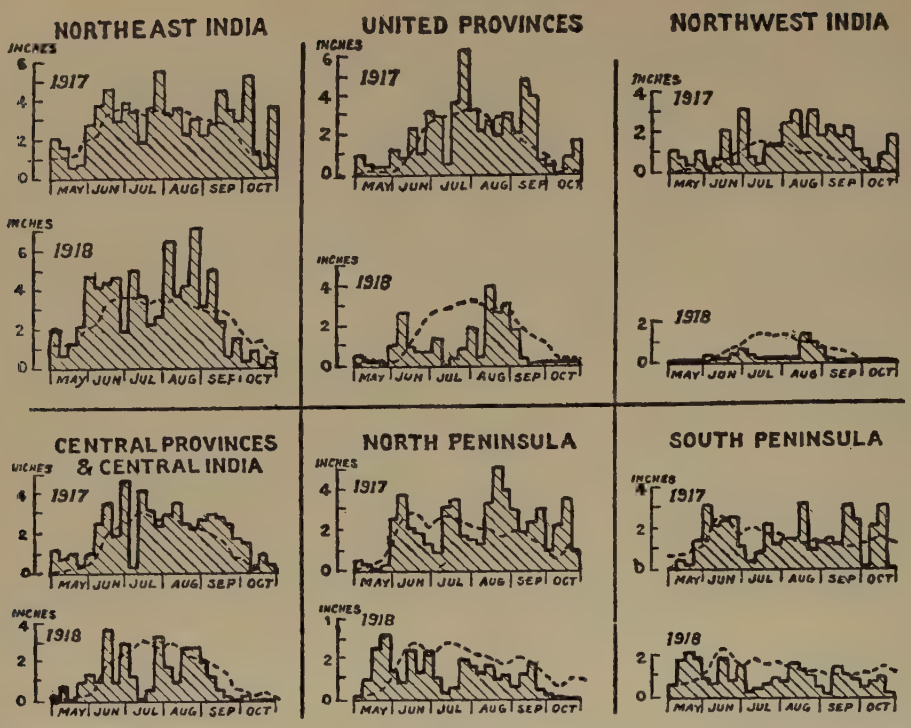


Figure 1. Progress of monsoon week by week in Famine Year (1918) and Excess Year (1917). Broken curves indicate the normal rainfall (reproduced by courtesy of the *Empire Journal of Experimental Agriculture*).

mind. And yet these two years of 1917 and 1918, chosen here to illustrate two well-remembered years of extremes, do also suggest that the extreme years have a certain persistence within themselves in the affected regions, and that the single parameter of total rainfall over an area has in their case a significance, favourable or ominous, which summarizes the season as a whole.

Let us look at a more generalized picture, another of Ramdas's diagrams, a monsoonal history of 75 years, picturing the total monsoon rainfall year by year in the separate 30 sub-divisions of the old India (now India and Pakistan). In this diagram (Fig. 2) an empty square denotes that rainfall has not deviated excessively from the average in the June-September period; a black dot means 'high' rainfall, a circle 'low' rainfall, the dividing line between one class and the other being roughly twice the mean deviation. From the rows we see there are some years when all sub-divisions fall into the 'normal' range, e.g., 1885 and 1906; other years when rainfall is abundant over most of the land, 1878, 1892-94, 1917; and years when the rainfall is scanty, of which the notorious famine years, 1877, 1899 and 1918 stand out conspicuously in their adversity. One notices also that, as in Pharaoh's dream, there can be series of fat years and lean years, e.g., the rainy years between 1887 and 1894, and the dry years from 1899 to 1907, the latter of which gave rise to widespread conjectures about a permanent worsening of the climate of India, especially in its north-western area. Explanations were being put forward for the change; some blamed it on deforestation, and some even on the increase of irrigation. When the Government asked for a report, Walker reviewed the evidence and concluded that the spell of lean years did not rise from human agency, but was part of the abnormalities in the larger movements of the atmosphere.

Dots and circles in Fig. 2 are not scattered haphazard along any row in the diagram, but tend to be bunched in a way that reminds us that neighbouring areas tell much the

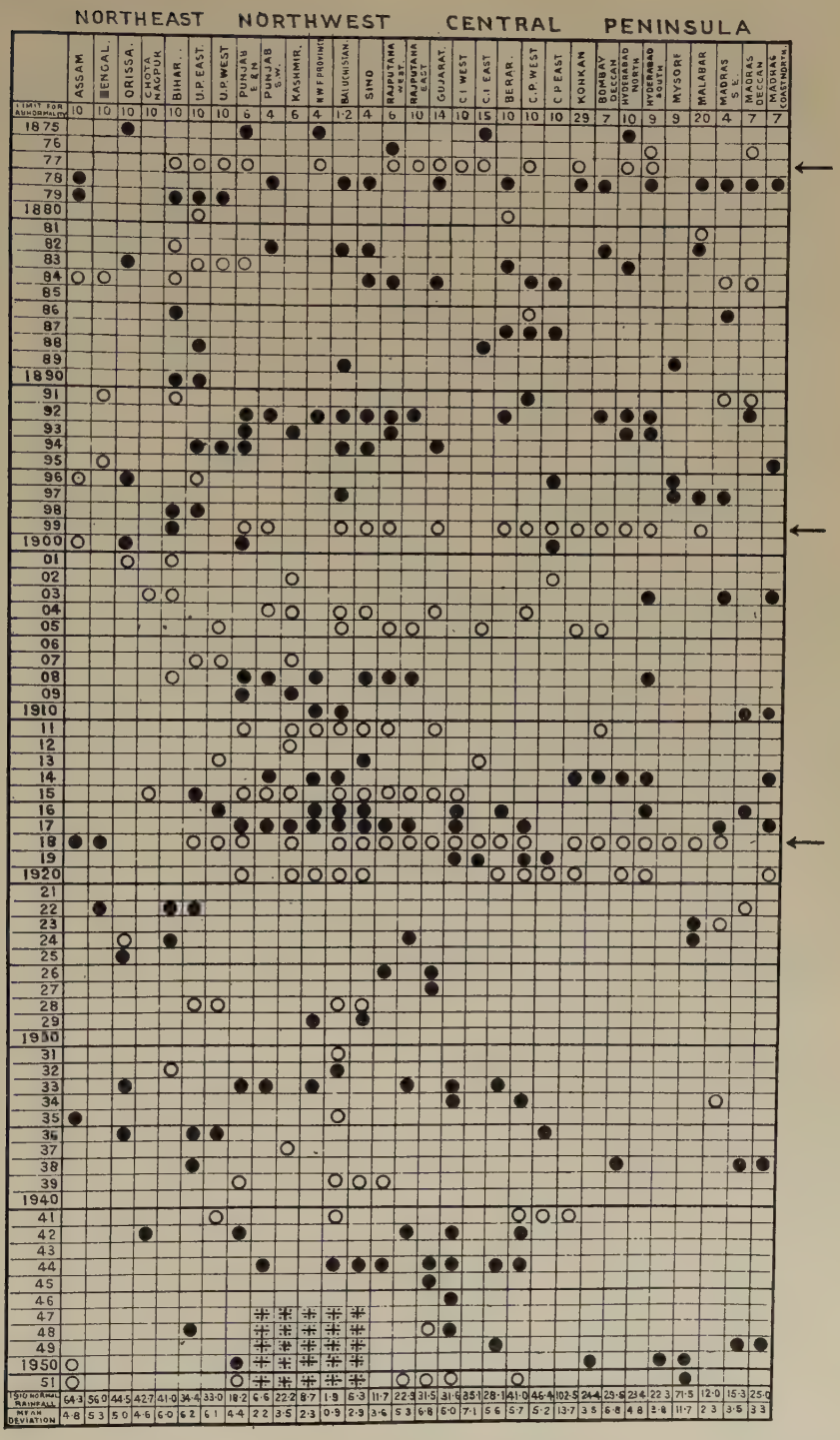


Figure 2. Droughts ○ and Floods ● in India (reproduced by courtesy of Dr. L. A. Ramdas).

the same story year by year. The years that were notable to the administrator for their abundance, normality or scarcity stand out in it. The diagrams as well as the relatively high standard deviations of Table 1 lend support to the view that a single figure of total monsoon rainfall for areas as large as NW. India or the Peninsula could be of considerable value to the adminstrator if forecast correctly, and may even be of some value to the individual farmer.

The other question concerned the extent of the search to be made, whether it should be limited, as before, to the monsoon area or be world-wide. The answer was obvious enough, because when Walker came to India fifty years ago, two discoveries of special relevance had been recently published. Hildebrandsson (1897) on examining the pressure data from 68 stations over the world had pointed out an opposition between Sydney and Buenos Aires. The Lockyers (1902) confirmed the see-saw between pressure around the Indian Ocean and in the Argentine and based a system of classifying pressures on this oscillation. Following up these suggestions Walker found a relationship between South American pressure in April and May and Indian monsoon rainfall in June to September. There is no *a priori* reason to expect this relationship between two events so far apart in space and time, and Walker's attempt to trace the connection through sea-ice or cold surface water moving eastwards from the South Atlantic to the South Indian ocean was inconclusive.

When the physical causes of an empirical relationship are not at all understood, it may be questioned whether one is justified in using the empirical results for prediction. It can be held that the only scientific basis for seasonal prediction is the possession of a complete and accepted theory of the general circulation, from which the effect in distant places of a seasonal variation in one region may be deduced and expressed by a formula. Unfortunately there was (and there is still) no agreed explanation of the general circulation, and certainly no quantitative theory whatever about deviations from the normal. To act upon the principle of a theoretical basis would have postponed all further attempts to advance seasonal prediction.

In the circumstances Walker frankly followed empirical methods, arguing that if, as seems to be the case, physically real relationships can be found by strict statistical methods, ignorance of their explanation should not stop him from using them, and that the more relationships we find empirically the more insight we shall have into the physical facts of the problem, and the more likely are we to find some ground on which to base a theory.

So an investigation begun with the narrow object of improving the Indian monsoon forecasts developed into an examination of world-wide variations of weather. The initial search was for evidence of the control exerted by sunspots on pressure, temperature and rainfall over the world. This was followed by the search for relationships between distant meteorological phenomena, contemporary and consecutive, which were described in a series of memoirs on ' World weather ' published by the Indian Meteorological Department and by this Society. The first World War indirectly aided that lengthy computational programme; and curiously enough the reason lay not in any urgent need for such data, but in the fact that the first World War damped down the normal activities in the Indian Meteorological Office, most of the scientific staff being away on military duties and the few remaining senior members being over-burdened with routine. So, to find full occupation for members of his junior staff, Walker arranged this long-term programme of computations.

From the rather bewildering mass of detail, a few broad generalizations emerged. The already familiar see-saw connection between the Azores and Iceland, and the similar fluctuation in the North Pacific Ocean, both appeared as expected quite prominently

in Walker's correlations, which employed three-monthly means in place of the more usual monthly units. The other fluctuation, the see-saw orginally discovered by the Lockyers, appeared to be a phenomenon on a much larger scale and may be described briefly as a tendency for air to be removed from the whole Pacific area from Tokyo to South America at the same time as air accumulates in and around the Indian Ocean and vice versa. Walker called this the Southern Oscillation. Its symptoms are not confined to the pressure see-saw but are evident also in the rainfalls of India, Java and Australia as well as in some temperature records. There is a certain association, too, between the two northern fluctuations and the southern. Walker called them oscillations, but it is necessary to realize that none of them has any regular periodicity, nor do they form the dominant part of the variance at any place. They are related to but not controlled by sunspots, and they are to be regarded, in Walker's words, 'as a systematic swaying of inter-connected world conditions, which is intensified or checked by solar changes.'

As just stated, Indian monsoon rainfall plays a part in the southern oscillation. Unfortunately for India, the southern oscillation in June-August – at the height of the monsoon–has many significant correlations with later events and relatively few with earlier events. To my mind the most remarkable of Walker's results was his discovery of the control that the Southern Oscillation seemingly exerted upon subsequent events and in particular of the fact that the index for the Southern Oscillation as a whole for the summer quarter June-August, had a C.C. of + 0·8 with the same index for the following winter quarter, though only of − 0·2 with the previous winter quarter. It is quite in keeping with this that the Indian monsoon rainfall has its connections with later rather than with earlier events. The Indian monsoon therefore stands out as an active, not a passive feature in world weather, more efficient as a broadcasting tool than as an event to be forecast. In recompense one might hope that the winter rains of north-west India would gain by this pecularity of the southern oscillation, but these winter rains are a feature rather of the temperate than of the tropical latitudes and fall into the same category as the Mediterranean winter rains. Accordingly, although the field of the southern oscillation extends beyond north-west India in July, it stops short of it in January.

On the whole Walker's world-wide survey ended by offering more promise for the prediction of events in other regions than in India. This did not necessarily mean that prediction in India was impossible. Already in 1908 Walker had indicated a few possible forecasting factors and had combined them in a multiple regression formula with a total C.C. of 0·58. As this formula had met with some success and some other possible factors had come to light, Walker revised his forecasting formulae in 1919 and again in 1924, just before leaving India. To exemplify the sort of information used by Walker, the factors selected in 1924 for the two main forecasts are tabulated in Table 2. Two other formulae were prepared for the second half of the monsoon, i.e., for the months August-September, for the same two areas; and one for the winter rains of NW. India. These

TABLE 2. FACTORS USED IN MAIN MONSOON FORECASTS (1924)

| Area | Factors | Period |
|---|---|---|
| Peninsula | South Rhodesia rain | Oct.-Apr. |
| June-Sept. | South American pressure | Apr.-May |
| $R = 0{\cdot}76$ | Dutch Harbour temperature | Dec.-Apr. |
| | Java rain | Oct.-Feb. |
| NW. India | South Rhodesia rain | Oct.-Apr. |
| June-Sept. | South American pressure | Apr.-May |
| $R = 0{\cdot}76$ | Dutch Harbour temperature | Mar.-Apr. |
| | Equatorial pressure | Jan.-May |
| | Himalayan snow accumulation | May |

last three, though they had C.Cs. of about 0·7, were considered to be less securely based than those for the whole monsoon. A sixth forecast was proposed for NE. India, but has seldom been used, because the variance of the rainfall there is so small a part of the total rainfall. Walker left suggestions about factors that should be kept under periodical review for possible future use, and indeed the forecast formulae have been revised on several occasions since 1924. It is not my purpose here to go into the details of these separate forecasts. What interests us is their performance. Were they usable and have they met with any success ?

Walker in 1930 applied a performance test to his 1908 formula. The relationship between calculated and actual rainfall for the 20 years 1909-1928 was substantially the same (+ 0·56) as it was for the years *before* 1909 (0·58); i.e., it had not only the correct sign but surprisingly had practically the same value. I say ' surprisingly ' because each factor in a forecasting formula is chosen originally out of a large number of possible factors, just because its value is high. Being selected for that reason it has a bias and its value in that particular sample of years is likely to be higher than it would be in an average sample of years. Also, it is difficult to believe that nature scrupulously fits a series of factors by a least-square method into a regression formula as we do. In general, therefore, one should expect performance to show a smaller C.C. than the calculated total regression coefficient.

The best test of the method is to compare the forecasts year by year with the actual rainfall. In general this has not been so simple as it sounds. The forecasts have often been stated in the form of ' normal or slight excess,' ' not very far from the normal ' or some other phrase, which without specific numerical statement of the limits that are forecast cannot be checked with certainty. This verification difficulty had not been realized until about 1930, and then it was decided that for a period of years the seasonal forecasts should state the numerical limits and the odds. The numerical form was retained until 1948 so that there is a period of eighteen years during which the check on errors is automatic. The odds aimed at were 4 to 1 against being wrong. Now, the layman who knows the variance of the seasonal rainfall but lacks any other forecasting rule, can attain the 4 to 1 degree of success by suitable use of the probability table. Any year he can say that the departure will lie within $\pm 1{\cdot}3\,\sigma$, where $\sigma$ is the standard deviation. Or, he may put his forecast into one of the simple forms : rainfall will be above $M_1$ or rainfall will be below $M_2$, where $M_1 = M - 0{\cdot}83\,\sigma$ and $M_2 = M + 0{\cdot}83\,\sigma$ and $M$ is the average rainfall. Each of these statements record a 4 to 1 random chance.

The calculated forecasts, on the other hand, have a standard variance $\sigma_1^2 = (1 - R^2)\,\sigma^2$ about the calculated $C$. When $(C + 0{\cdot}83\,\sigma_1)$ was less than $M$, the forecast took the form ' rainfall will be below the average '; when $(C - 0{\cdot}83\,\sigma_1)$ exceeded $M$, it generally stated that rainfall will be above the average. In each of these events the pure chance odds of success were of course 50 per cent, and there were two occasions of less than 50 per cent pure chance, when rainfalls considerably above the average (above 104 per cent and above 112 per cent respectively) were forecast, both as it happened correctly.

The more usual form of broadcast, however, has been dictated by what the public mainly want to know, namely, whether the monsoon will be bad or not – which allows one, on dividing the range of rainfall into three classes, to combine ' normal ' and ' above normal ' with general acceptance, but not to use the combination of ' normal or below normal,' which, though it has a statistical meaning, is of no practical value. Most of the forecasts, therefore, were put in the form – rainfall will be above a certain value $(C - 0{\cdot}83\,\sigma_1)$ – a typical statement being ' the total monsoon rainfall will be above 95 per cent of the average in the Peninsula and above 90 per cent of the average in NW. India.' These two particular forecasts have the pure chance of 62 per cent and 65 per cent respectively of

TABLE 3. MONSOON FORECASTS – COMPARISON OF ACTUAL NUMBERS OF ERRORS WITH NUMBERS ESTIMATED ON PURE CHANCE

| | Number of forecasts | Number wrong | Estimated errors on pure chance With normal distribution | With actual 1931-48 distribution |
|---|---|---|---|---|
| Whole monsoon | | | | |
| Peninsula | 16 | 1 | 6 | 4 |
| NW. India | 16 | 2 | 6 | 6 |
| Total | 32 | 3 | 12 | 10 |
| Second half monsoon | | | | |
| Peninsula | 14 | 3 | 5 | 3 |
| NW. India | 15 | 4 | 6 | 4 |
| Total | 29 | 7 | 11 | 7 |
| Winter | | | | |
| NW. India | 9 | 3 | 4 | 4 |
| Grand Total | 70 | 13 | 27 | 21 |

being correct; if unambiguous, they are also unambitious, the 80 per cent (or 4 to 1) standard aimed at on the regression formula being not so very much greater than the 65 per cent (or 2 to 1) odds on pure chance.

Looking over the eighteen years' forecasts I find there are indeed one or two which have as wide a latitude or tolerance as the layman would have to allow himself for a 4 to 1 (or 80 per cent) chance of success – forecasts which perhaps ought not to have been issued inasmuch as they were no improvement on the random-chance ones. The average odds in favour of the actual forecasts on a pure-chance basis amounted to 63 per cent for the Peninsula, 60 per cent for NW. India monsoon and 56 per cent for NW. India winter), and these figures have been used to determine the probable number of errors on a pure-chance basis in column 3 of Table 3 for comparison with the actual numbers of errors listed in column 2.

In eighteen years, at the rate of five a year, ninety forecasts in all might have been issued. On twenty occasions, almost half of them in winter, no forecast was issued; conflicting factors were the stated reason, and generally this occurred when the resultant bias from the regression formula was slightly negative and would have required the forecast to take the valueless form 'normal or below normal.'

For the whole monsoon season, out of 32 forecasts, 3 were wrong in comparison with the 12 errors probable on a pure-chance basis and the 6 errors allowable on the 80 per cent standard aimed at. For the second half monsoon, 7 wrong in 29 forecasts seems slightly below the 4 to 1 standard of 6 allowable but was an improvement on the pure-chance probability of 11 wrong. The results for the winter were below standard, 3 wrong out of 9, and little better than the pure chance. The grand total of 13 errors out of 70 forecasts is a slight improvement on the 14 errors allowable on the 4 to 1 standard, and considerably better than the 27 probable errors of pure chance. One is tempted to conclude that the eighteen years' experiment has shown that the forecasts were significantly successful.

Let us, however, look again at Ramdas's diagram of Flood and Drought years (Fig. 2) and notice the variance in the thirty years 1921-1950 compared with 1891-1920. The

TABLE 4. CORRELATION COEFFICIENTS

| | By multiple regression formulae up to 1924 | up to 1945 | By performance 1922-1951 |
|---|---|---|---|
| Whole monsoon | | | |
| Peninsula | 0·76 | 0·68 | + 0·59 |
| NW. India | 0·76 | 0·63 | + 0·20 |
| Half monsoon | | | |
| Peninsula | 0·66 | 0·63 | + 0·46 |
| NW. India | 0·70 | 0·68 | + 0·53 |
| Winter | 0·76 | 0·69 | + 0·40 |

recent period is astonishingly uniform; in particular the years 1931-1948 are not at all typical, having experienced much less drought than expected. The period favours the type of forecast actually adopted and would also have favoured the forecaster operating on a pure chance basis. Column 4 of Table 3 contains an estimate of how the pure chance odds of column 3 could be amended to allow for the actual distribution in the years 1931 to 1948. It shows that the results of the forecasts of the winter and second half monsoon may have been little or no better than pure chance; those for the whole monsoon do however seem to remain significantly better.

As a further check it would have been satisfactory to give you the correlation coefficients for the thirty years 1923 to 1952. With the changes in the formulae during the years, the data for some of the old factors are not readily available, and I have only a partial list of calculated values for the 1924 formulae, in addition to full data based on the 1940 and 1945 formulae. It seems that the correlation coefficients may have changed to the extent shown in Table 4.

The NW. India monsoon coefficient for 1922-1951 is the result of a particularly large disagreement in 1951 and poor agreement in the years 1926-1928; for 1929-1947 the correlation is nearer 0·6. The correlations between calculated and actual remain all positive, all of the correct sign but diminished in value. The same conclusion holds good in general for the individual factors. The signs of the major factors used in the 1924 formulae have not changed. Their values have decreased but not sufficiently to make us believe that the relationships are spurious. If the relationships had been spurious, the performance tests made since 1920 would have shown greater degrees of inconsistency than have been observed. Blanford's snowfall factor is the one with the most disappointing performance since 1920, but that may be wholly or partly due to a less efficient reporting and assessment of the state of the snow at remote spots in the Himalayas.

The question remains: are the relationships, though real, now so small that they are of insufficient value for the issue of useful forecasts? The most important need in monsoon forecasting is to pick out with a reasonable degree of success the years of low rainfall, the possible famine years. The worst monsoon years since 1931 have been 1937, 1938, 1939, 1941 and 1951 in NW. India, and 1939, 1941, 1951 and 1952 in the Peninsula. In these the proportion of wrong to all forecasts has been as high as 66 per cent. For the August-September rainfalls the worst years in NW. India have been 1937, 1938, 1939 and 1951, and in the Peninsula 1937, 1941, 1951 and 1952, in one of which (1941) no forecast was issued; these seven forecasts were unfortunately all in error. To ask a regression formula to select the extreme bad years is a very stringent test, which, it must be admitted, the actual forecasts have failed to pass in these twenty years. On the whole my experience inclines me to stress this failure in spite of the overall maintenance of the

80 per cent standard over the period. Until years of scarcity can be forecast with greater certainty the forecasts can give little help to the public at large, especially the forecasts for the second half monsoon and for the winter rains which have not been useful enough to justify their continuance. I am in favour of continuing the practice of forecasting for the whole monsoon each year, if only to keep the subject alive and in the hope that ideas for progress will emerge, and these forecasts should be sent to interested people. But whether they should continue to have the wide public distribution that they at present enjoy is open to doubt.

As to future work, we await new ideas. It is easy to suggest a re-examination of the various factors that almost passed in 1924 and were then put aside for further examination; this has been done in India. The real trouble is that experience now suggests that no combination of surface factors will in its performance prove to have a higher correlation than about 0·6. Upper-air data, when we have enough of them, may be of help, and later on will be worth putting through the statistical mill. The supposition of linearity in relationships is a handicap inflicted on us by our ignorance of the physical causes behind them. One hopes that persistent patterns of flow in the middle or upper troposphere may prove to be of prognostic value, but lacking a background of theory we still have to depend upon statistical methods, and there are insufficient years of data to test the significance of a variety of complicated relationships or patterns with their many degrees of freedom.

The most interesting index that has come out of all this work is that of the southern oscillation for the southern winter. Its activity was the basis of several forecasting formulae put forward by Walker, e.g., to foreshadow Australian rainfall for October to April, the rainfall of Ceara in Brazil, and Dawson temperature for December-February. These are the formulae most worth while examination in order to test how far performance bears out their promise, and to give us further information about the extent to which seasonal forecasts can be based upon surface data by the correlation method. Also, Walker's discovery of the Southern Oscillation's high correlation with itself six months ahead is certainly a matter for a seasonal forecaster to follow up. How real is this relationship between events six months apart ? If on re-examination it still stands the test, it merits a determined effort to find the physical causes behind it, and to provide seasonal forecasting with some solid ground to build upon.

The Meteorological Department of India owes a great debt to Sir Gilbert Walker for placing this subject of monsoon forecasting on a critical, impersonal objective basis. I personally am indebted to him for what I learned in discussion with him in India and later in correspondence. Of those who carried on the work after Sir Gilbert left India, I would like to mention in particular the late Rao Saheb M. V. Unakar, Rao Babadur V. Doraiswamy Iyer, and Dr. S. R. Savur.

551.594.221

# Lightning

By T. W. WORMELL
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Symons Memorial Lecture, delivered 29 April 1953

## 1. Introduction

To be honoured by an invitation to deliver the Symons Lecture is a mingled pleasure and ordeal; it is, I take it, part of the purpose of such an occasion that the lecturer should endeavour not only to give an adequate survey of his subject, but should attempt also to convey something of the fascination and tension which accompany the study and investigation of a natural phenomenon.

I can still remember vividly the first lecture I ever gave on the subject of lightning, when I was a young research student. The audience was the Science Society of my old school, a school more noted then for its classics than its science. The first question I was asked during the discussion, indeed the only question I can now recall, was a request that I explain an early meteorological observation, reported by the gentle Horace, of lightning striking from a clear sky :

> namque Diespiter,
> igni corusco nubila dividens
> plerumque, per purum tonantes
> egit equos volucremque currum,

Perhaps on this occasion I shall be let off more easily.

## 2. Early investigations

The scientific study of lightning is usually deemed to have begun just over 200 years ago with Benjamin Franklin. Living in an age when electrical experiments were becoming a drawing-room novelty, Franklin, with a group of friends belonging to a literary society in Philadelphia, began, perhaps in the very year in which the Leyden jar was discovered (1746), to dabble with the new amusement. They were encouraged thereto by the gift of an electrical machine and the opportunity of purchasing some other equipment.

Franklin was an amateur in the true sense of the term; he was also a great natural philosopher. In 1750, in a letter to his friend Collinson in London, he described with great clarity his scheme for the ' Philadelphia experiment,' which was to establish his fame as a scientist (Cohen 1941, p. 222) :

> To determine the question whether the clouds that contain lightning are electrified or not, I would propose an experiment to be tried when it may be done conveniently. On the top of some high tower or steeple, place a kind of sentry box, big enough to contain a man and an electrical stand. From the middle of the stand let an iron rod rise and pass bending out of the door, and then upright 20 or 30 feet, pointed very sharp at the end. If the electrical stand be kept clean and dry, a man standing on it when such clouds are passing low, might be electrified and afford sparks, the rod drawing fire to him from the cloud. If any danger to the man should be apprehended (though I think there would be none) let him stand on the floor of his box, and now and then bring near to the rod the loop of a wire that has one end fastened to the leads, he holding it by a wax handle; so the sparks, if the rod is electrified, will strike from the rod to the wire, and not affect him.

Although his plans were so clear, Franklin was in fact forestalled in the successful carrying through of this experiment, which was achieved by d'Alibard at Marly, near

Paris. A pointed rod, some 40 ft high, insulated and protected against the rain, had been erected. Instructions had been given to one Coiffier, an old soldier, in case d'Alibard should be away, and the village priest had promised to attend if a storm occurred. On 10 May 1752, between two and three o'clock in the afternoon, Coiffier suddenly heard thunder and rushed to the apparatus. He obtained sparks from the rod and summoned the priest who hurried to the scene through a hail-storm and himself repeated the experiment in the presence of a group of amazed villagers. Coiffier was despatched to Paris with a letter and, three days later, d'Alibard communicated the results to the Academy of Sciences. One month later, Franklin, who had not yet heard of d'Alibard's success, himself performed the experiment in Philadelphia, using a kite to raise the discharging point to a suitable height.

Franklin was not unique in suspecting that lightning and the electric spark were one and the same phenomenon. His achievement, based on a sound grasp of electrical principles, was the clear and successful planning of a simple and direct experiment to demonstrate that fact. Even so, many of us may feel some surprise at the tremendous impression made on the scientific world by the experiment, which was described by Joseph Priestley, a few years later, as ' the greatest that has been made in the whole compass of philosophy since the time of Newton.'

To our minds, the depth of Franklin's insight is perhaps more clearly seen in some further observations which he made a little later. A letter to Collinson, dated September 1753 (Cohen 1941, p. 267) describes how he had erected a discharging point on his house, in the autumn of 1752, together with a device (the ' electrical chimes ') which showed the occurrence of appreciable discharge current and indeed gave some indication of its intensity. During that same autumn he had observed – expressing his results in modern language – that intense electric fields at the ground accompanied showers as well as thunderstorms, that a sudden change of field accompanied a close lightning flash and that, not infrequently, the field was more intense immediately after rather than before a fairly close discharge. He next proceeded to investigate the sign of the current by allowing it to charge a Leyden jar and comparing this with another jar charged from his electrical machine. By 1753 he had concluded ' that the clouds of a thundergust are most commonly in a negative state of electricity, but sometimes in a positive state,' adding ' so that for the most part, in thunderstrokes, it is the earth that strikes into the clouds and not the clouds which strike into the earth.' The latter statement meant, in effect, simply that the flash transferred negative charge to the earth, although perhaps Franklin was giving a more material meaning to the terms positive and negative electricity than was justified.

Many of these facts were to be verified or rediscovered only in the twentieth century.

### 3. Fundamental investigations

If the scientific study of lightning began with Franklin, it would not, I think, be unjust to claim that its serious quantitative investigation had to wait some 160 years for the researches of C. T. R. Wilson, whom we rejoice, still, to number among our Fellowship. He was one of that remarkable group of workers which gathered round the young Cavendish professor, J. J. Thomson, in Cambridge, at the turn of the century, and which included, amongst others, Townsend, Rutherford, Langevin, Zeleny, Glazebrook, Shaw and Lempfert. While the impulse given by this group to the advance of atomic physics is the more widely known, their contributions to the study of the physics of the atmosphere should not be forgotten.

Wilson has described in his Nobel lecture (1927) how, during a stay of some weeks at the Observatory on Ben Nevis, in 1894, he was greatly impressed by the optical

phenomena associated with clouds which he observed there, and was led, by a desire to imitate them in the laboratory, to the construction of his first expansion chamber. It was the accidental discovery that ions, under suitable conditions, were efficient condensation nuclei, which caused him to deviate from this intention and to leave the highway of meteorology for the attractive by-road of atomic physics. Later, vivid experiences, while walking over the Scottish hills in stormy weather, were to play their part in attracting him to the intensive study of the thundercloud and of the lightning discharge.

Wilson made the first reliable and fairly direct measurements of the charge transported by a lightning flash; these observations were direct, not in the sense of actually intercepting the flash itself, but in measuring the sudden change in the vertical electric field at the ground due to a flash at a known distance (within say 20 km). In this investigation he used simple electrostatic principles, breaking away from the routine method of the ' potential collector ' and observing, instead, the sudden change in the bound electric charge on an exposed conductor of simple geometrical form maintained at zero potential; this might be, for example, an elevated sphere or a ' test-plate,' the latter effectively a sample portion of the earth's surface. Such observations, when made on an open site, lead directly to a measurement of the absolute value of the sudden change in the vertical electric field near the ground. It may be remarked, in passing, that the simple theory behind these measurements affords one of the best large-scale illustrations, available to a teacher, of the direct physical significance of the fundamental theorems of electrostatics. The charge $Q$ on a sphere of radius $r$ (15 cm, say), supported at a height $h$ (5 m, say), and maintained at earth potential, is given very nearly by the equation

$$\frac{Q}{r} - \frac{Q}{2h} + V = 0$$

where $V$ is the undisturbed potential at a height $h$ and is equal to $Fh$ if $F$, the vertical field, is constant over this small range of height. Again, the field $F$ and the charge $Q'$ on an earthed test-plate of area $A$ are related by the equation

$$F = 4\pi Q'/A$$

The distortion of the field, by the hut which protects the observing apparatus, is estimated by considering the standard text-book problem of a hemispherical boss on an infinite conducting plane. A record of $Q$ thus leads to an estimate of the magnitude of $F$ and of its variations.

Visual observations were made first with a simple gold-leaf electrometer; later, a capillary electrometer was employed to give a continuous photographic record of the variation of the electric field. This electrometer was used, in effect, as a coulomb-meter and was particularly well adapted to its purpose, giving effectively a continuous record of $Q$ while automatically maintaining the exposed conductor at earth potential; moreover it was dead-beat and sufficiently rapid in response to give the magnitude and some indication of the duration of the electrostatic field-change accompanying a lightning discharge, while ignoring completely the transient induction and radiation fields. There is a peculiar fascination in the beautiful records which this technique yields, and perhaps still more in actually watching the liveliness of the meniscus in this extremely simple system when a storm is active perhaps 20 km away. Some examples of records from storms at various distances are reproduced in Fig. 1. Modifications of this method make it possible to use it for overhead storms, even in the presence of heavily charged precipitation, or alternatively, and most conveniently by the use of an electronic amplifier, to extend the range up to some 150 km to 200 km (Wormell 1939, 1953). The practical limits are imposed, either by the presence of fairly rapid fluctuations of field due to causes other than lightning, or to disturbance from overhead power lines : in an open

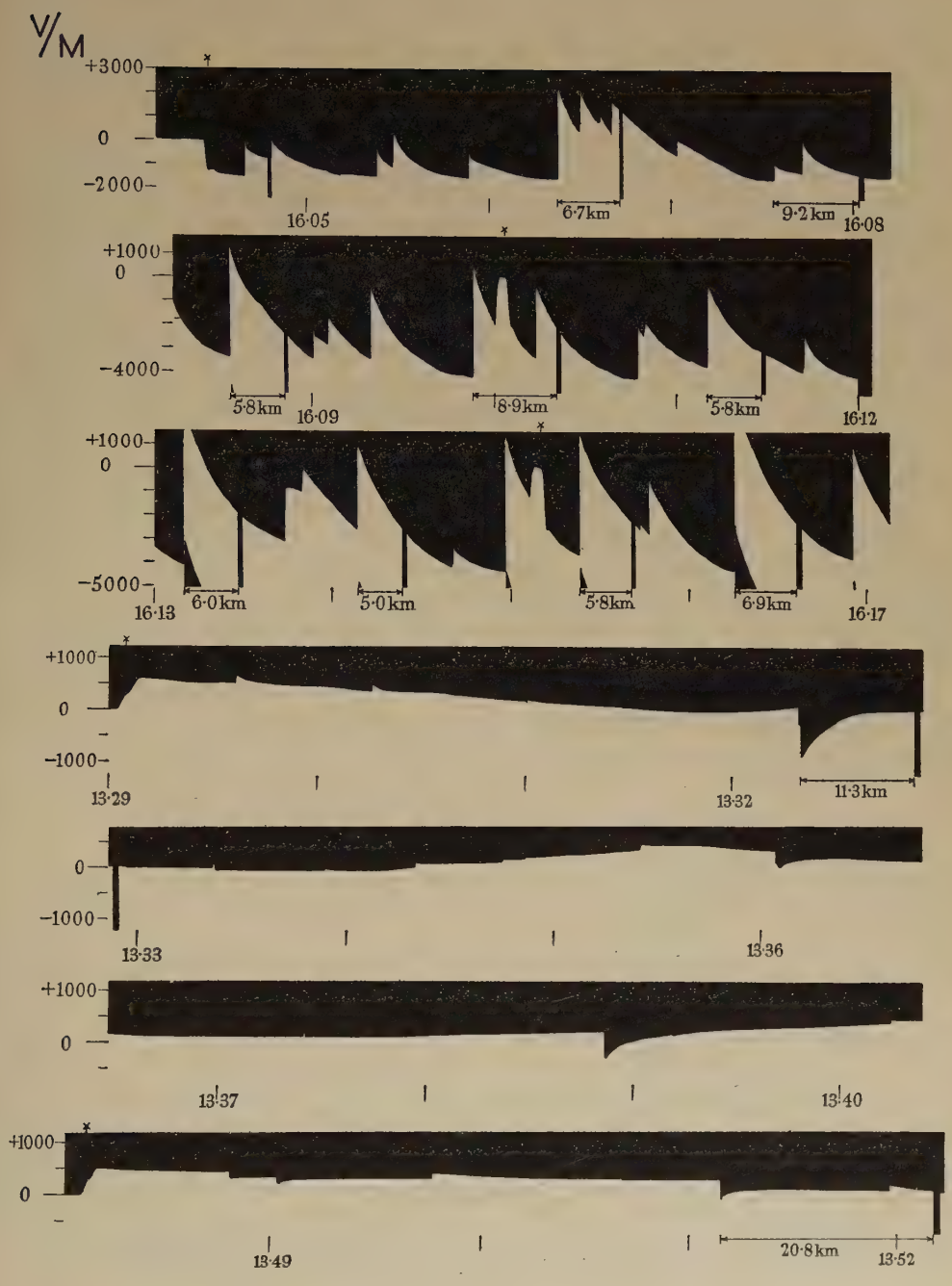


Figure 1. Examples of records of the vertical electric field made with a capillary electrometer in thundery conditions. The vertical black lines mark the beginning of thunder. The small crosses indicate determination of the actual field at that moment. The scale for the electric field shown on the left is in volts/metre.

field on the outskirts of Cambridge, for example, there is an alternating 50 c/s field with an amplitude of the order of 0·3 v/m.

In two classical papers (1916, 1920) and in his Franklin lecture (1929), Wilson discussed his observations by this technique and the ideas concerning the structure and physical processes of a thunderstorm which he had built on these results, and gave a great impetus to the study of this subject. A summary of some of these ideas has already been given in a recent article in this *Journal* (Wormell, 1953) and it will suffice now to recall that the results established included a determination of the electric moment destroyed by a typical lightning discharge and hence an estimate of the charge and potential difference involved, 20 C and $10^9$ v respectively, (figures revised in a more recent estimate by Bruce and Golde (1942) to 30 C and about $10^8$ v respectively); and the deduction that the electrical structure of a thundercloud was essentially bipolar, with the positive charge normally uppermost. This result, after some controversy, was to be confirmed and rendered more precise by observations with the alti-electrograph at Kew (Simpson with Scrase, 1937 and with Robinson, 1941). The model thundercloud, suggested by the Kew workers to fit their observations, consisted of a charge of + 24 C in a sphere of radius 2 km, centred at a height of 6 km, a charge of − 20 C in a sphere of radius 1 km centred at a height of 3 km, and a charge of + 4 C in a sphere of radius 0·5 km centred at a height of 1·5 km. While this last small positive charge, located near the cloud base, may well be of importance in the initiation of lightning discharges, it is still somewhat uncertain whether it is due to a fundamental charging process of the thunderstorm, or is of secondary origin, the charge coming from ions derived originally from point discharge at the ground. Observations by the Wilson technique established further that the commonest type of discharge to earth carries down negative charge, and showed also that the duration of the recorded field-changes was, frequently, markedly greater than the limit of resolution of the capillary electrometer and might exceed, perhaps, 0·2 sec without any obvious abrupt changes of slope.

## 4. Investigation of details by photography

Of recent years, owing in part to the possibilities inherent in new techniques, attention has tended to be concentrated on the study of the details of the discharge process. The first major advance in this field came from the determination of the detailed time-table of visible events in the discharge, by direct photography. This was made possible by the ingenious camera devised by C. V. Boys in 1902 (Boys 1926), the principle being that a distorted picture of the flash, taken with a camera in which the lens is in rapid and known motion relative to the film, and in a plane parallel to it, is compared with the picture taken simultaneously with an ordinary camera. A careful and laborious comparison of the two pictures enables an accurate time-table to be attached to all the details visible in the photographs. The principle of this method is illustrated for an idealized lightning discharge in Fig. 2. The actual lightning discharge includes processes occurring with a great diversity of speeds, and with a great range of luminosities, the more rapid events being fortunately the more luminous, in general: in order to obtain a reasonably complete picture of the events happening in a single flash several cameras of different speeds must be employed simultaneously. The rapid Boys camera will indicate the order and time separation of events only one or two microseconds apart.

This device was first used successfully by Schonland and his collaborators in a beautiful series of investigations in South Africa, from 1933 onwards (Schonland 1938). The series of events in a flash to earth, which was thought to be typical, but which would, perhaps, be more accurately described as the type most easily photographed, may be summarized as follows:

A leader stroke proceeds from cloud-base to ground in a time of the order of 0·01 sec; this is in an intermittent process which advances in a series of luminous steps, each starting from the point reached by the end of the preceding step, progressing for a distance of the order of 100 m, in a time of the order of 2 $\mu$ sec, and the luminosity then dying away again. During the advance of a step, the whole of the previously-formed channel is lit up by luminosity, which travels rapidly down it from the cloud-base, but this glow is considerably fainter than that in the newly-forming part of the streamer. The time interval between successive steps is of the order of 50 $\mu$ sec. The advancing stepped leader follows a tortuous path, and may branch, on its way towards the ground; it determines the form of the main discharge channel. The precise form of this irregular path and the presence or absence of branches are presumably controlled by the behaviour of the electric field, at each successive moment, in the immediate vicinity of the advancing tip, and this, in turn, will be dependent on the distribution of space charge in the region between the cloud and the ground. It is not until a quite late stage that it is determined which point on the ground is to be struck or, indeed, which of the various branches is to be the one to reach the ground. The advancing streamer progresses until it is about 5 m from the ground when it is met by a streamer coming from the earth. (This positive streamer from the earth is increased in length if a prominent object is suitably placed on the ground to initiate it and, in the extreme case of a very high structure such as the Empire State Building, New York, the stroke is normally initiated by a stepped leader which starts from the top of the building and progresses upwards all the way to the cloud-base.) In the normal case, when the descending leader makes effective contact with the conducting ground, there follows immediately the rapid and violent return stroke, in which an extremely vivid but brief luminosity travels at very high speed up the channel from the earth to the cloud-base, and out along the various branches, from that which has now become defined as the main stalk of the discharge. The duration of the return stroke, the time, that is, taken by the bright luminosity to travel from earth to cloud-base, is of the order of 40 $\mu$ sec, but a faint luminosity persists in the channel for a much longer period.

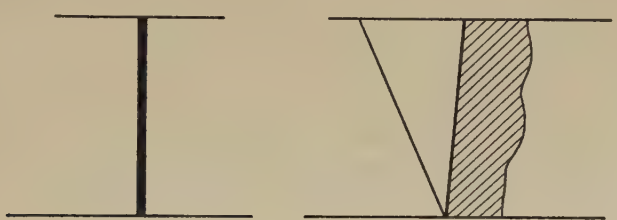


Figure 2. Diagram to illustrate the principle of the Boys camera. (*a*) Represents the picture, taken with a stationary camera, of a vertical flash between cloud-base and ground. (*b*) The picture obtained with a camera in which the lens is moving so as to displace later events to the right. It represents (not to scale) a dart leader, a bead of light, travelling downwards from cloud to earth, followed by the rapid spread upwards of the luminosity of the return-stroke and a fainter luminosity (represented by the hatched area) persisting for a little time.

Further strokes may occur down the same main channel, but usually showing no branches, at intervals of the order of 0·1 sec. Each consists of a dart leader, a luminous 'bead' of light, travelling down the channel in a time of the order of a millisecond, followed by a return stroke, with a time-scale similar to that of the first return stroke, but with a tendency to possess a more complicated structure in its later stages.

Later investigations by the South African workers resulted in the recognition of two different types of initial leader process. That already described, now termed the $\alpha$ type, had, throughout its path, a fairly uniform effective velocity of propagation, which was normally between 100 km/sec and 1,000 km/sec, a typical value being 200 km/sec. The steps were often fairly uniform, both as regards their size and the time interval between them. When variations were observed, the step length was found to be greater the greater the interval since the last step, but the relation was not linear. The apparent (i.e., projected) length of a step was usually between 10 m and 200 m and the interval between successive steps between 30 $\mu$ sec and 100 $\mu$ sec.

In the second or $\beta$ type of initial leader, the velocity of advance was at first considerably higher, ranging roughly from 600 km/sec to 3,000 km/sec, a typical value being 1,500 km/sec; in general this type was more luminous than the $\alpha$ type and the steps were longer. At some stage of the path to earth, associated often with marked branching, the velocity of advance dropped sharply to a value of the order of 100 km/sec or less, and, at the same time, the steps became short and faint. It had thus now become, in effect, a feeble $\alpha$-type process and even, we might suggest, on occasion, a faint continuously advancing streamer. The total duration of the $\beta$-type leader is normally, despite the higher speed of its initial portion, appreciably greater than that of the $\alpha$-type process.

In their earlier papers, Schonland *et al.* (1938) reported that $\alpha$-type leaders preceded the first stroke to earth for some 65 per cent of flashes, the remaining 35 per cent having a $\beta$-leader. Early field-change observations, both in this country and in South Africa, seemed to be in agreement with this identification of the $\alpha$-type leader as the commoner one. More recently, Malan and Schonland (1951a), as a result of field-change studies, have stated that at least two-thirds of the initial leaders are of the $\beta$-type; their Boys camera studies however only disclose leaders of this type in some 10 per cent of cases, the implication being that in the remaining cases the fast initial portion is within the cloud and hidden by it, the leader when it becomes observable being a faint comparatively slow process with small steps, if such are present at all.

To sum up, therefore, the results of these photographic investigations, the typical flash to earth is initiated by a leader process descending from the cloud to the earth with an effective velocity in the range 100 km/sec to 1,000 km/sec ($\alpha$-type) or, in the $\beta$-type, initially 500 km/sec to 3,000 km/sec, but later dropping sharply to 100 km/sec or less. It is usually accepted, following Schonland, that this effective velocity is the actual velocity of a continuously descending ' pilot-streamer ' which is too faint to be photographed, the bright steps representing an intermittent brightening of luminosity and enhancement of ionization in a channel which has already been formed. The velocity of advance of the luminosity in an individual step is of the order of 50,000 km/sec.

The return stroke follows, its vivid luminosity moving up from the ground with a velocity of the order of 20,000 km/sec to 150,000 km/sec, the higher velocities tending to occur in the earliest part near the ground.

Subsequent strokes are preceded by a dart leader which travels down the existing channel continuously, if the time interval from the preceding stroke is not too long, with a velocity of the order of 2,000 km/sec, and is observed as a ' dart ' of light, the main luminosity being within 50 m of the advancing tip; this is again followed by a return stroke.

The current carried by the initial leader process is known from field-change studies, to be discussed later, to be of the order of 100 a; the peak current in the return stroke is known from various lines of evidence to be, in an average case, of the order of 20,000 a though it may in rare cases exceed 100,000 a.

### 5. Investigation of details by field-change studies

A systematic study of the varying electrostatic fields due to flashes at moderate distances (30 km to 150 km), of which I hope that a detailed account will shortly be published, has been carried through at Cambridge by Pierce (1950). Some of these results have been summarized in a recent article (Wormell 1953). At these distances the electric field, during a discharge, effectively pictures the varying electric moment of the charge distribution as it is changed by the flash. This investigation, which involved continuous recording of the field in disturbed weather conditions, was made with oscillographic equipment, particular care being taken to ensure that slow changes (up to a duration exceeding one second), were recorded faithfully; the details of changes occurring in less than one millisecond were not resolved.

The most striking feature of the records is the large part of the total change in electric moment which occurs quite slowly in the great majority of flashes, (cf., Wormell 1953, p. 10). In the case of the typical flash to earth, bringing down negative charge (about one flash in three is of this nature in English storms), there are usually several abrupt changes of slope, the field change including one or more rapid steps termed $R$ portions each with a duration of perhaps 2 m sec (Fig. 3). Preceding the first of these is a slow $L$ portion, with a duration of the order of 0·1 sec; between the $R$ steps, the field change, $J$ portions, may be zero or comparable with the magnitude of the $L$ portion and the durations of the $J$ and $L$ portions are also of the same order. A slow $S$ field change, again similar in character to the $L$ and $J$ changes, may follow the last $R$ step.

In the case of cloud discharges, the $R$ steps are absent, and any changes of slope in the recorded field-change are quite gradual; the duration is typically of the order of 0·2 sec.

Figure 3. Diagram of a typical electrostatic field-charge due to a flash to earth, showing $L, R, J$ and $S$ portions of the change of field. Total duration about 0·2 sec.

It is natural to associate the $L$ portion of the field-change in such records with the initial leader disclosed by photography, although the durations reported for the latter are considerably smaller. The $L$ field-changes recorded by Pierce seem to fall fairly naturally into two types which, by analogy with the photographic results, are termed $\alpha$ and $\beta$ types. When the field changes steadily and uninterruptedly during the leader process the field-change is described as an $L$ ($\alpha$) change; if, on the other hand, the rate of change of field falls away and becomes nearly zero for an appreciable period before the next $R$ step, it will be described as an $L$ ($\beta$) change; cf., Fig. 4. While the similarity with the two main types of initial leader discovered by photography is fairly close, the time-scale of the field-changes is considerably slower. The mean duration observed for an $L$ ($\alpha$) field change was 0·05 sec and for an $L$ ($\beta$) change, 0·18 sec; with this we may

compare the reported durations for $\alpha$ and $\beta$ leaders of 0·01 and 0·02 sec respectively (Schonland *et al.* 1938).

The *R* steps which stand out so sharply on these records have usually a duration which is perceptibly greater than the minimum resolving time and may attain several milliseconds; this is of the order of 100 times the duration of the brilliant luminosity in the return stroke between the earth and the cloud base. The *R* part of a field-change will clearly include the effects of anything in the nature of a dart leader, of the violent current surge in the return stroke, and the slower neutralization of charge in the cloud which ensues. In particular, any statement of the change of electric moment associated with the *R* step of a field-change must not be equated with the effect of the return stroke in its more restricted sense.

The *J* portions and the final slow portions of field-changes are similar in general appearance and properties to the *L* portion. The sign of all the different portions of a field-change associated with a flash to earth, observed at these distances, which are well beyond the complications of reversal zones, is normally positive if the magnitude is appreciable; in other words, every part of the change of field is of the sign which could be interpreted as due to the lowering of negative charge.

In the case of flashes to earth including only one stroke (the commonest type in this country), the average changes of electric moment associated with the *L*, *R*, and *S* portions are 35, 29, and 25 C km respectively.

The characteristic steps which are so prominent in many Boys camera studies of leaders, and which indeed made their photography possible, do not produce any perceptible electrostatic effects. The sudden spurts of current associated with them, however, cause appreciable radiation fields whose existence can readily be detected on field records of the type which have been described, with suitable adjustment of the brilliancy of the recording spot. It is found that the stepped structure is by no means a universal feature of the *L* portions of field-changes, or of those due to cloud discharges; it only appears on some of them and then, usually, only for small parts of the whole slow *L* change of field, and clearly requires special conditions for its development. The typical initial leader process would appear to be, over much of its path, a slow continuously advancing streamer.

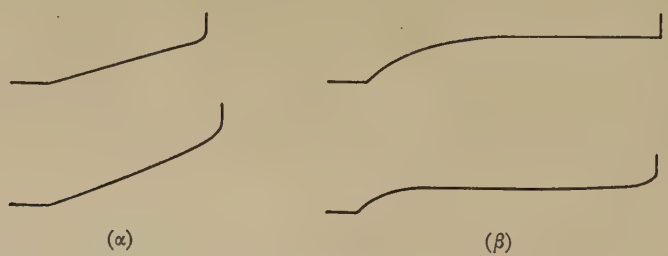


Figure 4. Diagrams of *L* ($\alpha$) and *L* ($\beta$) field-changes. Mean duration of *L* ($\alpha$) type 0·05 sec, of the *L* ($\beta$) type 0·18 sec.

The apparent discrepancies in time-scale between these field-change observations, made in this country, and the South African photographic studies do not, in all probability, represent a real difference in the nature of the typical discharge processes in the two cases. More recent observations in South Africa (Malan and Schonland 1951) have disclosed the existence of slow changes of field between the strokes (interpreted as due to ' junction-leaders ') very similar to those indicated here. It would appear that photographic observations select the brighter leaders, which tend to be the more rapid ones

and those most frequently 'stepped,' and to miss the slower and fainter examples. The spasmodic and unreliable occurrence of the stepped process also suggests that studies which involve the opening of a camera shutter by a trigger actuated by a repetitive disturbance (as has been used for photography in daylight) are likely to be strongly selective in their results. With field-change observations, an untrue picture is obtained unless great precautions are taken to include the effects of the slow parts of the change of field, a condition which was not satisfied in much earlier work; this of course increases considerably the difficulties of observation since it makes the equipment sensitive to all manner of unwanted disturbances. Here too, if the method of recording involves the triggering of some part of the equipment by the initial portion of the disturbance, great caution must be exercised in accepting the results as a true sample of the phenomena being studied. Many workers with such techniques must have watched with astonishment while their apparatus ignored many very close discharges and was yet responding to numerous atmospherics from storms hundreds of miles distant.

Our picture of a typical flash to earth must thus be somewhat modified; the initial process is a slow streamer which proceeds downwards towards the ground with a velocity which may have a wide range of values from about 15 km/sec upwards. This streamer often advances continuously over much of its path but may develop, particularly when the velocity is rather high, the typical stepped structure. This first leader distributes a considerable negative charge along its path, its branches, and in the surrounding air and frequently causes a change of electric moment of some 30 C km. This, together with its typical dimensions and duration, imply that it carries a current of the order of 100 a. After the completion of the ensuing return stroke, the conducting channel and its immediate surroundings will be nearly at earth potential and will hence be positively charged. New leader processes may now start out from near the top of the first stroke and, if one of these links it with a further concentration of negative charge, a second stroke to earth will occur down the same channel, this consisting of dart leader and second return stroke. The common phenomenon of a flash consisting of several strokes is thus explained, following Schonland (1938), as the discharge to earth of several discrete charged volumes; the suggestion that the leaders for the subsequent strokes start out from near the top of the first channel, being in fact positive leaders, is due to Bruce and Golde (1942). The time intervals between successive strokes in a flash are of the same order as the duration of the initial *L* field-change and, assuming that the leaders involved all travel at comparable speeds (there is in fact very little definite information on the properties of positive leaders on this scale), the spatial separation of the various discrete charge centres would be of the same order as the height of the first charge removed to earth.

A leader stroke transports considerable charge and hence causes a marked change in electric moment and a significant change of field, at the distances we have been considering, unless the motion of the charge is nearly horizontal. In English storms, in about 75 per cent of flashes to earth which include 2 or more *R* steps, the magnitude of the *J* field-change is imperceptible. It follows that, in these cases, the path of the junction leaders was nearly horizontal and the several discrete charge centres successively discharged to earth by the strokes of a single flash all at nearly the same height. In the remaining cases the sign of the *J* changes indicated that normally the heights of the negative charges removed to earth in the successive strokes steadily increased. In the storms of the Johannesburg region of South Africa, studied by Malan and Schonland (1951b), these proportions are just reversed, the typical flash to earth there, removing to earth in its successive strokes negative charge from a considerable range of heights. Another well-marked difference is to be found in the number of successive strokes in an average flash. In the English observations which have been discussed, the commonest

flash to earth consists of a single stroke and only some 35 per cent have more than two strokes. In the lower latitude of Johannesburg, the commonest number of discrete strokes is three, and the more complicated flashes to earth are decidedly more common than in English storms.

## 6. The physical processes of the discharge

The lightning discharge is a great electric spark and the physical interpretation of some of its main features is fairly clear.

The initial leader streamer (and presumably the junction leaders) are the only processes known to propagate into ordinary unionized air. The ' pilot-streamer ', a faint streamer advancing continuously with a velocity equal to the effective speed of advance of the intermittent stepped leader, was originally a hypothesis put forward by Schonland which neatly explained many of the features of the stepped leader process, and indeed Meek and Perry (1944-45) considered that its existence in the lightning discharge was not definitely established. The recent field-change observations which have been discussed suggest that, for much of its path, the typical initial leader is something very like Schonland's hypothetical pilot-streamer, without the stepped process superposed, and indicate that this streamer can propagate with velocities as low as 15 km/sec. Schonland maintains that the fundamental process involved in this streamer is an electron avalanche, the tip advancing at a speed, which is dependent on the electric field in its immediate vicinity, and is sensibly the same as that the individual electrons there. The electrons multiply as a result of ionization by collision with the air molecules in their path. This explanation involves the existence of a minimum speed below which the streamer cannot propagate. The new low speeds of leaders, indicated by recent field-change studies, would seem to leave this explanation theoretically just possible. The case of a positive leader, which seems, although information is scanty, to travel with a speed of the same order of magnitude, would require a considerably modified picture. It is possible that, in either case, ionization ahead of the advancing streamer by ultra-violet light, emitted from just behind the tip, may play an important role.

The other types of streamer, stepped leader, dart leader and return stroke, all travel along a pre-ionized channel and the advance of luminosity which accompanies a current surge indicates the spread of a condition (ionization and excitation of molecules) along the channel. This may happen at a very great speed, dependent on the electric field and ionization density in the channel; the apparent velocity is proportional to $N^{1/3}\,vd$ where $N$ is the electron deposit in the pre-ionized path, $v$ the electron-drift velocity just ahead of the tip and $d$ the distance beyond the tip for which the field is sufficient to cause ionization by collision. The velocity with which the luminosity spreads along the channel may thus be very much greater than the speed of any material particles in it.

The stepped-leader process is a somewhat puzzling phenomenon. Schonland has attempted to explain it in terms of the ageing (decrease of ionization) in the older part of the channel while Bruce (1944) has considered the corona losses and a transition between glow and arc conditions in the advancing streamer. Neither explanation is completely satisfying.

An advancing leader streamer, carrying a current of the order of 100 a, forms effectively a conductive connection with the charge centre from which it originated and, when it has progressed a considerable way to the ground, there will exist an intense electric field not only at its tip but also at its edges, and considerable corona losses are to be expected (Bruce 1944); the situation is similar to that of a metallic conductor carrying a current at a high potential. Pierce (1952) has discussed the advance of a streamer from this point of view and has shown that a consistent picture can be obtained, with a reasonable

value for the corona constant; further, various commonly-observed forms for the $L$ field-change can be neatly explained by reasonable assumptions as to the distribution of field between the cloud-base and the ground.

## 7. Atmospherics and the location of thunderstorms

The lightning discharge besides causing the electrostatic effects of the changes in charge distribution in the thundercloud, also acts, effectively, as a powerful radio transmitter; the field strength in the signal, commonly called an atmospheric, which it radiates, becomes progressively stronger, relative to its electrostatic effects, at greater and greater distances. The loud crashes produced in a radio receiver by an individual discharge, at moderate distances, are a very familiar phenomenon, and the effects of the numerous thunderstorms which are always active in some parts of the earth constitute the limiting factor in many problems of telecommunication.

Atmospherics, are of course, of great practical importance in meteorology as they afford a means of locating distant regions of thunderstorm activity. The bearing from which the disturbance has arrived can be determined by standard methods of radio direction-finding, using the directional properties of a loop receiving aerial. In the Sferics organization of the Meteorological Office, which maintains four widely-separated direction-finding stations in the British Isles, this affords a rapid and reliable method of locating the position of individual lightning discharges, within say 2,000 km, as a matter of routine observation. It involves, however, a fairly elaborate organization and a network of stations which must be manned and linked by telephone lines. It becomes, therefore, a matter of some interest to enquire what possibilities exist for modifying and simplifying such a procedure; whether, in particular, the form of the disturbance contains within itself unambiguous information as to its distance of origin so that the location of a flash could be fixed by observations from a single station. To avoid misconception, I should perhaps make clear that I am merely discussing whether this is possible. I am not putting forward a scheme which is comparable, at present, in speed or reliability with the existing Sferics system.

The radiated signal from a lightning discharge is proportional to the rate of change of the current, and is only of significant intensity when associated with rapidly-varying current surges. This means in practice, that the only important atmospherics, observed at considerable distances, arise from intermittent leader processes (or their equivalent in cloud discharges), and from the return stroke of a discharge to earth; the latter gives the strongest signals. The signal from a stepped leader is a complicated and irregular disturbance of considerable duration (Fig. 5 (a)), which is propagated without major change of form to considerable distances. The disturbance originating from a return stroke, on the other hand, is, in comparison, a brief and simple pulse, but the signal received therefrom, at a great distance, is profoundly modified by the properties of the intervening medium and may have a prolonged oscillatory or repetitive character.

There are two alternative ways of analysing the propagation of the disturbance between the conducting earth and the ionosphere; they are, of course, physically equivalent provided the phenomenon is analysed adequately (Budden 1951). The first considers the two boundaries, of which one at least is an imperfect conductor, as forming a wave-guide, and examines the properties of the different wave-guide modes by which a disturbance can be propagated. This involves a Fourier analysis of the pulse which excites the disturbance, and then, for each Fourier component, a consideration of the intensities with which the various modes are excited and their differing attenuations; finally, at the receiving station, a summation over the different modes and an integration over the

different frequencies must be performed. Under some conditions, a very small number of modes is propagated with small attenuation and this method, despite its seeming complexity, may then be the most direct way of deducing the form of the disturbance to be expected. Under other circumstances, when the ionosphere acts as a very good conductor, more modes are effective and such an analysis would be very laborious. In such circumstances, however, a discrete pulse, the original signal, may be pictured as being reflected from the ionosphere with only a slight change of form and the signal actually received at a distant station pictured as consisting of this pulse, which has travelled by the direct route, followed by a series of echoes which have been reflected once, twice, . . . $n$ times from the ionosphere and $(n - 1)$ times from the ground respectively. The signal would thus consist of a repetition of similar pulses of diminishing size and with a time spacing which depends on the values of $D$, the distance from source to receiver, and $h$, the height of the reflecting ionosphere, and which, for given values of these quantities, is readily deduced from simple geometrical considerations. If the curvature of the earth be neglected the distance travelled by a signal which has been reflected $n$ times from the ionosphere is $2n\{h^2 + (D/2n)^2\}^{1/2}$ and the time interval between the arrival of echoes of order $n$ and $(n + 1)$ is simply the difference between the two paths divided by the velocity of light. In fact, when $D$ is large, the calculation must be made for a curved earth; the time intervals between the successive pulses increase, at first rapidly, later more slowly, and ultimately, if echoes of sufficiently high order can be observed, approach asymptotically the constant value $2h/c$.

It has been known for a considerable time that many night-time wave forms (Fig. 5 (d)) consist of a repetitive pattern whose time-spacing is in agreement with this simple picture. For observations in southern England, the common form originating from a return stroke, at night, is of this type, when the storm is in the quadrant from S to E. of the British Isles, and for any distance out to several thousand kilometres. The length of the train of echoes tends to increase as the night goes on and may exceed forty. A comparison of the measured time intervals with the predictions of the simple theory normally leads, with this type of wave form, to reliable determination of both $D$ and $h$, the latter rarely departing significantly, at night, from about 85 km. When darkness has been established for some time at ionospheric heights over the whole path, many pulses can be measured, and such an analysis is possible, in the SE. quadrant for flashes out to some 8,000 km.

For storms to the SW. of the British Isles a remarkable difference is found (Caton and Pierce 1952). Wave forms of the type just discussed are only observed, even at night, when the storm is fairly close, within some 1,500 km. At greater distances the wave form approximates to a smooth oscillation (Fig. 5 (c)), the total number of peaks observed being much smaller. The appearance of such wave forms does not suggest a series of discrete echoes, nor, in general, does the time spacing of the peaks fit the simple geometry of successive reflections. Indeed, no method of analysis has yet been devised which will give a reliable estimate of the source distance for the night-time sinusoidal forms. No detailed explanation has as yet been evolved for this curious difference between the waveforms from storms to the SE. and to the SW. of the British Isles when the distance exceeds about 1,500 km, nor indeed do we know with certainty whether the significant feature is the actual geographical location of the storm or the direction of propagation of the disturbance, relative, for example, to the magnetic meridian.

During daylight, the common return stroke wave form (Fig. 5 (b)), is a rather smooth oscillation with a small number of peaks. The appearance of the wave form scarcely suggests discrete echoes and the limited number of peaks makes it difficult to decide definitely whether the peak spacing fits the echo picture, and would, in any case only

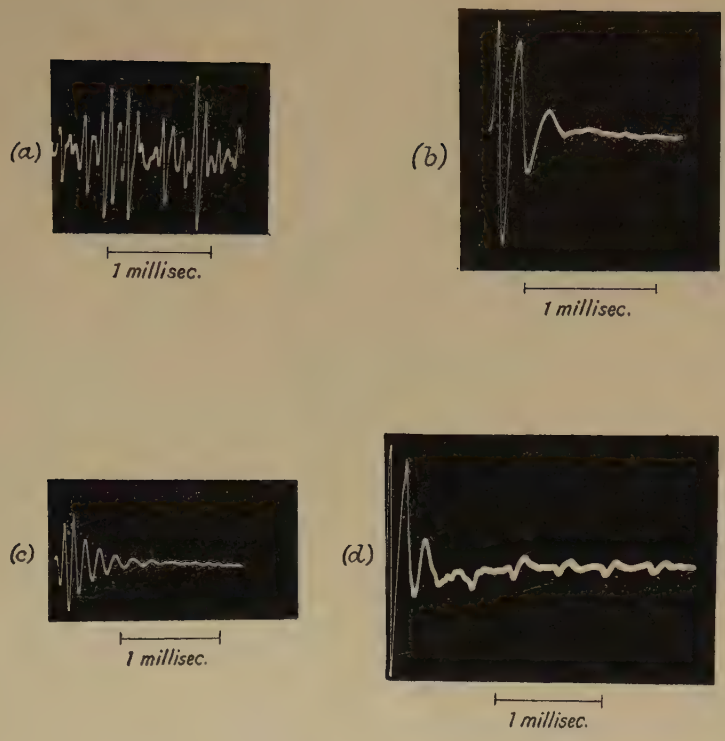


Figure 5. Four common types of atmospherics :

(*a*) The irregular type associated with the stepped-leader process and occurring either as precursor to a regular form from the return stroke or from a cloud discharge. Total duration may be much greater than that of the picture.

(*b*) The typical regular day-time waveform from moderate distance.

(*c*) Typical sinusoidal waveform from distant sources to SW. at night.

(*d*) Typical reflection type waveform at night from source to SE.; the regular pattern may persist for considerably longer than the photograph.

permit a reliable determination of $D$ for fairly close storms. It would seem that, for storms within say 1,000 km, the peak spacing is not far from that predicted by the reflection picture although the small deviations appear to be systematic (Caton 1952); the deduction of $D$, when it is within this range, may thus be possible from simple wave form analysis, possibly with an empirical modification of the ordinary reflection theory.

To sum up, single-station observations will yield by measurements with loops and a vertical aerial, the bearing of a lightning flash without ambiguity of sense. Straightforward analysis of the observed wave forms, by measurement of time intervals, will give, at night, the distance of storms to the SE. with an accuracy which on most occasions probably exceeds that attainable by a Sferic D.F. system and out to a considerably greater range. Flashes to earth, at night, within about 1,500 km can be located whatever the direction of the storm, but beyond this distance to the SW. a reliable estimate of distance cannot, in general, be obtained by this method. During daylight, similar methods will

locate, probably, all active storms within perhaps 1,000 km or a little more. A definite decision as to the reliability and regularity with which such information could be obtained can probably only be reached by operating such a station regularly for a period, with the sole object of locating storms; the question has been argued *ad nauseam*. If the results of such a trial were encouraging, it would be worth putting a considerable effort into the technical problems of the best methods of recording for this specific purpose, and into the development of more expeditious methods of selection and analysis of the resulting records. It is perhaps worth pointing out that the method discussed is considerably more powerful during the hours of darkness and might conceivably be a useful supplement to a D.F. network during periods when it was not desired to keep all the stations manned. An experienced observer will have a fair notion of the approximate distances of the active storms centres and even of their directions (E. or W.), and, in particular, will usually notice the outbreak of storms in a new area, simply by watching the incoming wave forms on the oscillograph screen, at night.

This discussion has dealt only with one particular method of analysing wave forms, the simplest. There are various other possibilities, some of which are promising from this point of view. Two only will be mentioned. The energy spectrum of the disturbance, which can be studied by recording simultaneously the response of several circuits tuned sharply to different frequencies, shows characteristic changes with distance of propagation (Bowe 1951) and merits more detailed study. Again, many atmospherics have very prominent low-frequency components (*c.* 400 c/s), the so-called ' slow-tail ' which is propagated with a group velocity appreciably less than that of the higher-frequency disturbances which we have been considering. Thus there is a delay between the beginning of the main atmospheric and the first sign of a low-frequency disturbance which increases linearly with distance and at a different rate by day and by night (Hepburn and Pierce 1953). While this behaviour is quite systematic, there is a considerable scatter between individual wave forms and it remains to be seen whether a practicable means of distance determination can be based on it.

## Acknowledgments

I should like to express my gratitude for many discussions with Dr. E. T. Pierce while preparing this lecture; I have drawn on certain aspects of his observational results in advance of their detailed publication. The atmospherics reproduced in Fig. 5 were recorded in Cambridge by Dr. P. G. F. Caton.

I should also like to take this opportunity of acknowledging the indebtedness of Cambridge workers in this field, over the last few years, to the Director of the Meteorological Office, and the workers of its Sferics system, for much time and trouble taken in locating for us the individual lightning discharges whose effects we were studying.

## References

The first three references below comprise a short bibliography of the subject :

Bruce, C. E. R. and Golde, R. H. 1942 ' The mechanism of the lightning discharge and its effect on transmission lines,' *J. Instn. Elect. Engrs.*, **88** (Part II), p. 487.

Meek, J. M. and Perry, F. R. 1944-45 ' The lightning discharge,' *Rep. Progr. Phys.*, Physical Society, X, p. 314.

Schonland, B. F. J. 1938 ' The lightning discharge,' Halley Lecture, Oxford University Press.

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|---|---|---|
| Bowe, P. W. A. | 1951 | *Phil. Mag.*, **42**, p. 121. |
| Boys, C. V. | 1926 | *Nature*, London, **118**, p. 749. |
| Bruce, C. E. R. | 1944 | *Proc. Roy. Soc.* A, **183**, p. 228. |
| Budden, K. G. | 1951 | *Phil. Mag.*, **42**, p. 1. |
| Caton, P. G. F. and Pierce, E. T. | 1952 | *Ibid.*, **43**, p. 393. |
| Caton, P. G. F. | 1952 | Dissertation (Cambridge University). |
| Cohen, I. B. | 1941 | *Franklin's 'Experiments and Observations on Electricity,'* Harvard University Press. |
| Hepburn, F. and Pierce, E. T. | 1953 | *Nature*, London, **171**, p. 837. |
| Malan, D. J. and Schonland, B. F. J. | 1951a | *Proc. Roy. Soc.* A, **206**, p. 145. |
| | 1951b | *Ibid.*, **209**, p. 158. |
| Pierce, E. T. | 1950 | Dissertation (Cambridge University). |
| | 1952 | *Nature*, London, **169**, p. 562. |
| Schonland, B. F. J. | 1938 | *Proc. Roy. Soc.* A, **164**, p. 132. |
| Schonland, B. F. J., Hodges, D. B. and Collens, H. | 1938 | *Ibid.*, **166**, p. 56. |
| Simpson, G. C. and Robinson, G. D. | 1941 | *Ibid.*, **177**, p. 281. |
| Simpson, G. C. and Scrase, F. J. | 1937 | *Ibid.*, **161**, p. 309. |
| Wilson, C. T. R. | 1916 | *Ibid.*, **92**, p. 555. |
| | 1920 | *Phil. Trans.* A, **221**, p. 73. |
| | 1927 | Nobel Lecture, Stockholm. |
| | 1929 | *J. Franklin Inst.*, **208**, p. 1. |
| Wormell, T. W. | 1939 | *Phil. Trans.* A, **238**, p. 249. |
| | 1953 | *Quart. J. R. Met. Soc.*, **79**, p. 3. |

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# A photoelectric raindrop spectrometer

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Summary

An instrument which gives a continuous, automatic record of the size distribution of raindrops at ground level is described. The light scattered by the drops in falling through an optically defined area of a light beam is focused on to a photomultiplier cell and the resulting voltage pulses are sorted electronically into eight different size groups. The spectrometer is used to study the variation of the drop-size spectrum with time and with the rate of rainfall. Calibration curves and some specimen results are presented.

## 1. Introduction

The size distribution of raindrops, its relation to the rate of rainfall and its changes with time are matters of considerable meteorological interest. The methods of obtaining this information which have been most widely used in the past, have involved the measurement of stains produced on a dyed filter paper (Lenard 1904, Defant 1905) or the impressions made on sugar- or soot-coated nylon screens, (Mt. Washington Report 1951, Blanchard 1952). A novel technique was introduced by Bentley (1904) and used successfully by Laws and Parsons (1943); the raindrops fall into finely-sieved flour to produce dough pellets whose sizes are a function of the diameter of the original drops. These methods are, however, very laborious, not capable of giving a continuous record, and are inconvenient when the drop-size distribution is changing rapidly.

Bowen and Davidson (1951) have built an instrument which gives a continuous record of the drop-size distribution. The drops fall through an orifice into a wind tunnel and are deflected horizontally through a distance inversely proportional to the cube of their diameters. They then strike a moving dyed filter paper, their sizes being determined from the diameters of the stains. The stains are thus sorted into size groups but the numbers in each group have to be counted. This instrument has three main disadvantages; it has a sampling area of only 1 in.$^2$, it can deal only with vertically falling drops and is suitable only for drop sizes from 0·3 mm to 1·5 mm in diameter.

Cooper (1951) utilizes another technique in which the drops impinge on a microphone and their mass is determined from the magnitude of the impact. A balloon-borne instrument has been developed to telemeter an automatic record of the drop-size distribution to the ground. There are, however, many inherent difficulties in the method. It is difficult to obtain uniform response over a microphone diaphragm which is large enough for edge errors to be unimportant; the noise level increases rapidly with the size of the diaphragm so that the detection of small drops is prejudiced; errors arise from shattering of the larger drops on impact. Moreover, it is a serious disadvantage for the response to be proportional to the momentum, i.e., to approximately the fourth power of the diameter.

In the instrument now to be described, the raindrops are allowed to fall through a parallel, rectangular beam of light, the sampling area being optically defined. The light scattered at an angle of about 20° by the falling drops is focused on to the cathode of a photomultiplier cell, each drop producing a voltage pulse of amplitude roughly proportional to the cross-sectional area of the drop; there is some departure from this law at small drop sizes because of the non-linearity of the amplifier. The voltage pulses, corresponding to various drop sizes, are sorted by electronic circuits into eight different size groups, the number in each group being recorded continuously on a four-figure telephone-message register.

## 2. Design of the instrument

The design of the instrument centres around the choice of sampling area, which must be large enough to record a representative sample of drops during a shower of a few minutes' duration, but not so large that coincident counts will be numerous. The sampling area must be well defined and the optical system designed so that a drop of given size will produce the same response at all points in the sensitive area, except, of course, at the very edges. A principal feature of this device is the optical delimitation of the sampling area so that its use is not restricted to vertically falling drops and the results are not vitiated by the splashing which occurs with mechanically-defined systems. At present the sampling region has an area of 72 $cm^2$, the beam being 5 cm wide and 0·5 cm deep (see Fig. 1), but the width may be reduced by a mask during either heavy rain or light drizzle, when the maximum counting rate of the registers is in danger of being exceeded.

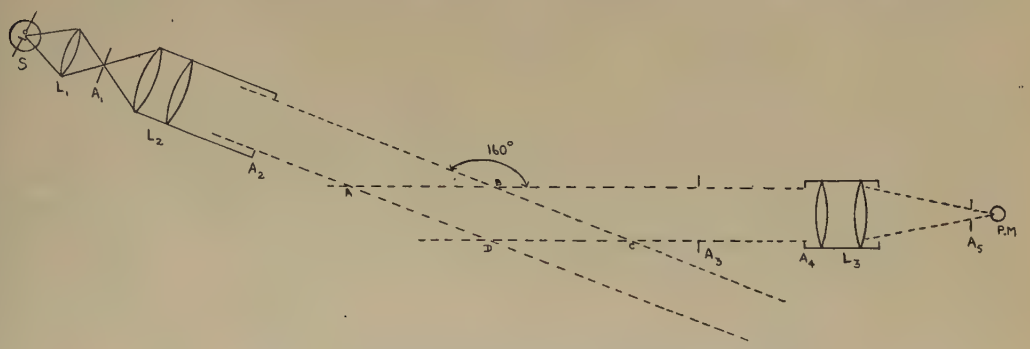


Figure 1. The optical system of the raindrop spectrometer.

Within the above limitations it is desirable to employ as large a sampling area as possible in order to minimize the effects of the edge errors. Drops falling through the edges of the beam are registered as smaller than their true size, but as the effective sampling area for each drop size can be found by calibration, a suitable correction can be made to the readings of the recorder.

Drops of diameter greater than 100 $\mu$ can be sorted into eight groups, the range of diameters recorded in each group being pre-selected according to the type of precipitation. For rain of light and medium intensity the drops are counted in 7 ranges of 0·25 mm, from 0·5 mm to 2·25 mm, the eighth group containing all drops of diameter greater than 2·25 mm. For light drizzle and heavy rain, the size groups are extended downwards and upwards respectively, so that the whole range of drop diameters from 100 $\mu$ to 6 mm can be covered.

## 3. Construction and operation

A schematic diagram of the optical system is shown in Fig. 1. The light source S is a d.c.-operated, air-cooled, high-pressure mercury arc lamp, a sharp image of which is produced at the small circular aperture $A_1$ placed at the focus of a carefully-corrected lens system $L_2$ from which emerges a parallel beam. A rectangular slit $A_2$, 5 cm × 0·5 cm produces a rectangular parallel beam which is monitored for uniform distribution of intensity by traversing with a narrow slit placed in front of a photocell. The detecting unit consists of a telescope system $L_3$ at the focus of which is placed the cathode of a sensitive photomultiplier cell PM. Slits $A_3$, $A_4$ screen the photomultiplier from stray illumination and protect the lenses from the rain. The sampling area ABCD is thus defined by the intersection of the ' beams ' from the collimator and telescope and, being remote from both, erroneous counts due to splashing are avoided.

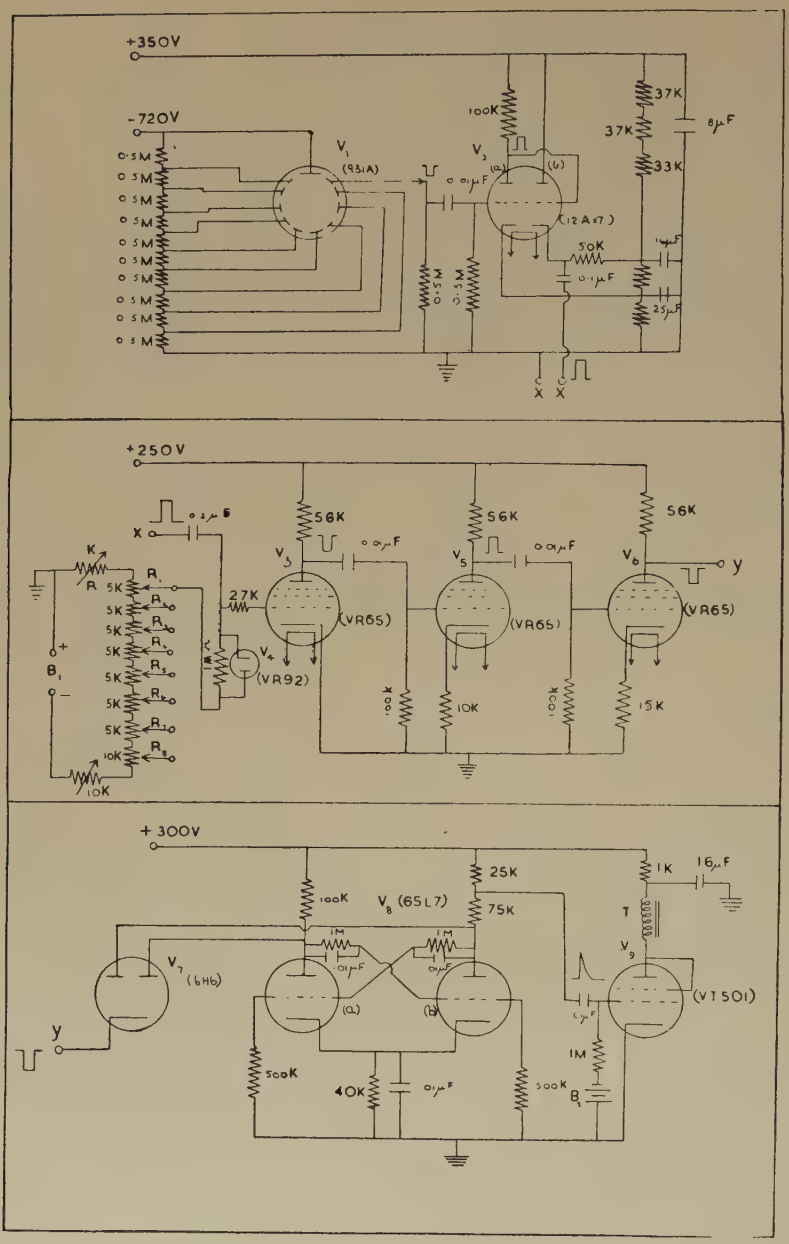


Figure 2. Circuit diagram of photomultiplier, amplifier and one complete sorting and counting channel. The polarity of the pulses appearing at the output of each stage is shown.

The passage of a drop through the sensitive area produces at the cathode surface of the photocell a light pulse of duration from 0·5 to 5 m sec, depending on the fall velocity of the drop, and causes a negative voltage pulse across the 0·5 m $\Omega$, resistive load (see Fig. 2). This pulse is then amplified about forty-fold and inverted by $V_2$ (a) and is taken from the cathode follower $V_2$ (b) as a positive pulse along a coaxial cable to the sorting and counting units housed in the building. There are eight discriminating and counting channels, each of which will pass pulses (i.e., record drops), above a certain pre-determined size. Each channel consists of a simple single-stage

discriminator $V_3$, a two-stage amplifier-inverter $V_5$, $V_6$, a scale-of-two circuit $V_7$, $V_8$ and a driving stage $V_9$ in the anode circuit of which is the relay counter T.

The bank of eight counters together with a ninth which acts as a recorder for a sensitive tilting-bucket raingauge giving 100 counts for 1 mm of rain, is photographed together with a clock at either regular half- or one-minute intervals. In the recording camera, photographic paper is driven at a speed of 5 cm/min, being stopped for a fraction of a second when the exposure is made. At the same time, a cam closes a pair of contacts which switch on lamps to illuminate the counters and clock to give less than 0·1 sec exposure. Counting of the drops in the different size groups is additive, i.e., channels 1, 2, 3 . . . . record respectively $N_1$, $N_1 + N_2$, $N_1 + N_2 + N_3$ . . . . thus $N_1$, $N_2$, $N_3$, the numbers of drops in the different size groups, are obtained by simple subtraction. From the photographic record it is possible to obtain the drop-size spectrum as a function of time and the rate of rainfall.

## 4. Calibration

The instrument is calibrated with water drops of standard size. Drops larger than 2 mm diameter are produced from carefully-ground glass tips attached by rubber tubing to a burette and drops of smaller diameter are produced by a modified version of the microburette described by Lane (1947). The procedure is to allow the drops to fall at different points through the sampling area and to measure the amplitude of the voltage pulses appearing across the cathode of $V_2$ (b) on a calibrated cathode-ray oscillograph. Frequent checking of the calibration is made in order to correct for ageing of the mercury lamp and the photomultiplier.

The reliability of the instrument was initially checked by comparing the indicated drop-size distributions with those obtained simultaneously by the filter-paper technique. The results of such a comparison are shown in Fig. 3. A further check is provided by comparison of time integrations of the readings of the spectrometer and the rate of rainfall recorder.

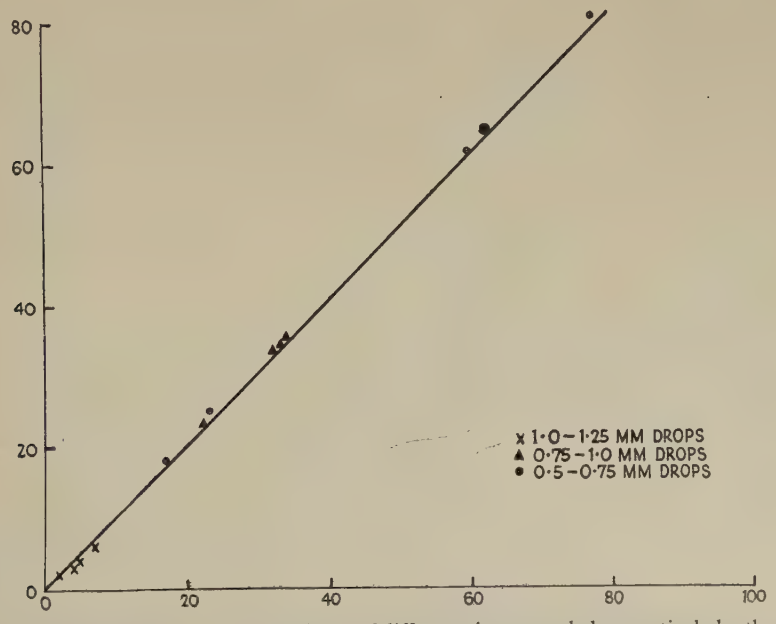


Figure 3. A comparison of the numbers of drops of different sizes recorded respectively by the spectrometer and by the filter-paper technique. Each point represents the number of drops of a particular size recorded by the two methods during an interval of 20 sec. The results were obtained from 4 filter papers exposed at different times during a light shower. The plotted points are to be compared with the straight line indicating perfect correlation.

## 5. Performance

The sensitivity of the instrument is limited mainly by the intensity of extraneous light falling on the cathode of the photomultiplier and the inherent noise in the photocell itself. During the night, when the extraneous light is reduced to a minimum, drops of diameter down to 100 $\mu$ can be readily detected; still smaller drops can be measured if the photomultiplier is refrigerated in order to reduce the internal noise. In daylight, drops falling outside the sampling area are also registered, but if the general level of illumination is low and steady, the bias on the discriminators can be adjusted so that only the drops falling through the sampling area are registered. Drops of diameter 100 $\mu$ can still be detected. At present the spectrometer cannot be used during showery weather in the daytime, because of the variability in background illumination. Tests are now being made to overcome this limitation by using only the ultra-violet component of the light from the mercury arc and an ultra-violet sensitive photomultiplier.

Errors in counting and classification of raindrops may arise from the fact that drops falling near the edge of the beam produce smaller responses than those falling nearer the centre, and by two drops falling through the beam at the same time to produce only one pulse. As the sampling area is chosen so that not more than 10 drops pass through per second, and their smallest remain for only about 5 m sec in the beam, the proportion of coincident counts will be negligible.

The edge errors are two-fold. Drops arriving at the edge will be only partly in the beam; the effective sampling area is thus diminished by 2·7 per cent and 7 per cent respectively for drops of diameter 0·75 mm and 2·5 mm. The intensity of the beam falls off towards the edges; calibration shows that this leads to a 6 per cent diminution of the sampling area which is the same for all drop sizes. By careful calibration of the sensitive region with drops of different sizes, the ' effective ' sampling area for each drop size is determined and appropriate corrections are made to the readings of the counters.

## 6. Results

A few results, sufficient to illustrate the working of the instrument, are shown in Figs. 3, 4 and 5. As already indicated, Fig. 3 shows a comparison of the drop-size distributions as determined by the spectrometer and the filter-paper method for short

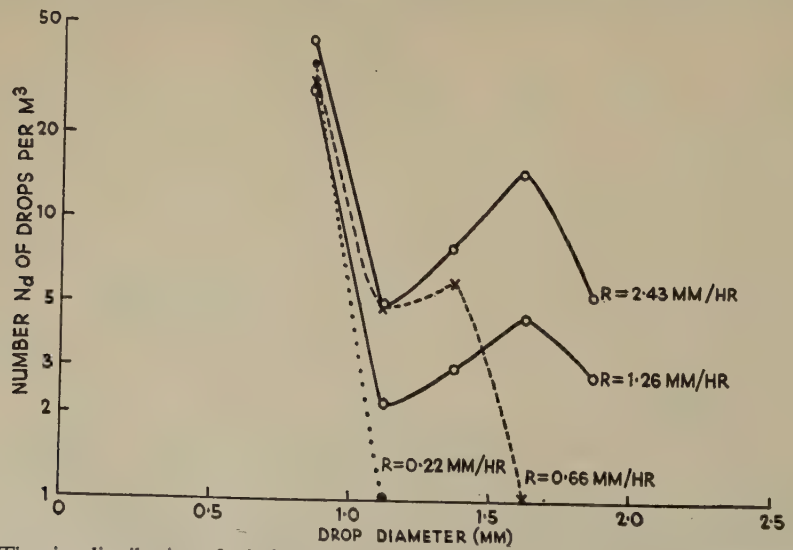


Figure 4. The size distribution of raindrops for various rates of rainfall during a prolonged period of light showery activity.

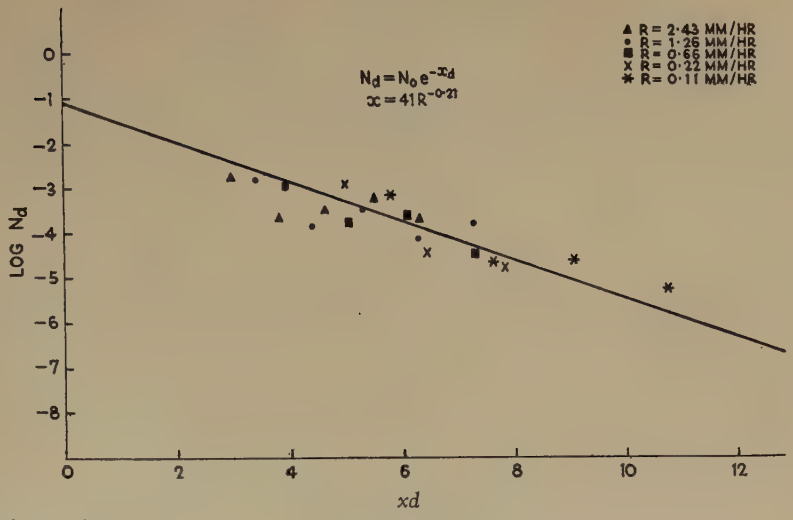


Figure 5. Comparison of results on drop-size distribution obtained with the spectrometer and the law given by Marshall and Palmer.

time intervals during a shower. The cumulative volume of precipitated water as indicated by the spectrometer and by the rainfall recorder over a period of some minutes during a light shower agreed within about 5 per cent, but instantaneous values showed greater discrepancies because of the lag of the recording gauge.

In Fig. 4 is plotted the spatial concentration $N_d$ of drops of diameter $d$ against $d$, for various rates of rainfall occurring in a prolonged period of light showery activity. It will be seen that the distribution curves, each covering a period of about one minute, appear to have two modes; curves of similar form have been obtained by Blanchard (1952) for showers in Hawaii.

Marshall and Palmer (1948) have proposed a distribution law of the form $N_d = N_0 e^{-xd}$ where $x = 41\,R^{-0.21}$, $R$ is the precipitation rate in mm/hr and $N_0$, a constant $= 0{\cdot}08$. In Fig. 5 data obtained with the spectrometer during periods of different rainfall intensity are plotted in the form of $\log_{10} N_d$ against $xd$. It will be seen that the experimental results conform quite well to the distribution law of Marshall and Palmer which is represented by straight line.

It is intended to use the spectrometer in conjunction with a 3-cm radar to study the characteristics of precipitation in relation to the structure and life history of the cloud.

## Acknowledgments

The authors are greatly indebted to Mr. E. G. Jennings for constructing the mechanical parts of the instrument and to Mr. E. K. Bigg for valuable suggestions concerning the design of the electronics. One of us (R.R.) wishes to express his thanks to Andhra University for the award of a research scholarship.

## References


| | | |
|---|---|---|
| Bentley, W. A. | 1904 | *Mon. Weath. Rev.*, U.S.W.B. **32**, p. 450. |
| Blanchard, D. C. | 1952 | *Mar. Met. Rep. Woods Hole Ocean. Inst.*, No. 52/99. |
| Bowen, E. G. and Davidson, K. A. | 1951 | *Quart. J. R. Met. Soc.*, **77**, p. 445. |
| Cooper, B. F. | 1951 | *Austr. J. App. Sci.*, **2**, p. 43. |
| Defant, A. | 1905 | *K. Akad. Wiss. Math. Naturwiss.*, **114**, p. 585. |
| Lane, W. R. | 1947 | *J. Sci. Instr.*, **24**, p. 98. |
| Laws, J. O. and Parsons, D. A. | 1943 | *Trans. Am. Geophys. Un.*, **24**, p. 452. |
| Lenard, P. | 1904 | *Met. Z.*, **21**, p. 248. |
| Mt. Washington Obs. | 1951 | *Res. Rep.*, Jan. 1951. |
| Marshall, J. S. and Palmer, W. M. | 1948 | *J. Met.*, **5**, p. 165. |

551.510.41

# The origin of atmospheric nitrous oxide

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SUMMARY

New observational data are discussed which indicate that :
(a) Nitrous oxide is uniformly mixed with the atmosphere up to 10 km and perhaps to 40 km, and that the volume mixing ratio is $3{\cdot}5 \times 10^{-7}$.
(b) Bacterial reactions in the soil can supply nitrous oxide to the atmosphere at a rate sufficient to compensate photo-chemical decomposition.
(c) The formation of nitrous oxide by homogeneous chemical reactions is probably of less importance.
(d) The average magnitude of the nitrogen cycle between earth and atmosphere is probably not less than $10^{11}$ molecules $cm^{-2}$ $sec^{-1}$.

## 1. INTRODUCTION

Nitrous oxide is the only oxide of nitrogen whose presence in the atmosphere has been established by direct observational evidence, and the problem of its origin is of great interest.

Adel (1951) holds the view that the gas has been emitted from the soil as a result of bacterial reactions, and it is of interest to note that, from a knowledge of such processes, Beijerinck and Minkman (see Verhoeven 1952) predicted in 1910 that nitrous oxide should be an atmospheric constituent. Bates and Witherspoon (1952) have cast doubts upon the suggestion that such reactions are the main source of this gas in the atmosphere, since this would appear to imply that nitrous oxide is a major end-product of soil de-nitrification, a conclusion which they were reluctant to accept. Seeking an alternative source, they considered possible chemical and photo-chemical reactions and found one which is free from objections and which might also proceed at a sufficient rate.

We will re-examine these problems in the light of new observational data.

## 2. THE VERTICAL DISTRIBUTION AND THE RATE OF DESTRUCTION OF ATMOSPHERIC NITROUS OXIDE

Six records of the 7·8 $\mu$ band of nitrous oxide in the solar spectrum were obtained on aircraft flights over southern England (Yarnell and Goody 1952) at altitudes between 3 km and 8 km. Prof. Adel kindly sent us seven spectra taken in Arizona at various times of the year at an altitude of 2·21 km. Details of the analysis of these spectra are given by Goody (1953). If the individual rotation lines have the dispersion line shape, the total nitrous oxide above the level of observation can be determined if it is assumed that the mixing ratio is constant. By observing at different levels this assumption can be tested for self-consistency. The main objection to the use of low-resolution prismatic spectra for quantitative work is that overlapping bands are difficult to distinguish. It is known that methane absorbs at 7·6 $\mu$ but nevertheless the observed solar spectra were very similar in shape to laboratory spectra of the 7·8 $\mu$ nitrous oxide band alone, and for this reason it was assumed that the methane absorption could be neglected. The aircraft results were consistent with a constant mixing ratio of $2{\cdot}7 \pm 0{\cdot}8 \times 10^{-7}$ by volume between 3 km and approximately 10 km. The spectra from Arizona were consistent with a volume mixing ratio of $4{\cdot}0 \pm 0{\cdot}5 \times 10^{-7}$. These results may be compared with the mass-spectroscopic measurements of Slobad and Krogh (1950) at ground level in Texas, which gave a range of values from 2·5 to $6{\cdot}5 \times 10^{-7}$. Having regard to the scatter of the results

all these observations appear to be consistent with a mean value of $3{\cdot}5 \times 10^{-7}$, and they indicate that the mixing ratio is substantially constant up to about 10 km.

There is reason to believe that nitrous oxide will be uniformly mixed up to higher levels. Bates and Witherspoon have calculated the natural life time of a molecule of the gas to photo-dissociation and find the results shown in Table 1.

TABLE 1

| Altitude, (km) | 10 | 20 | 30 | 40 | 60 |
|---|---|---|---|---|---|
| Life time, (days) | 4,000 | 800 | 50 | 20 | 10 |

The time constant for mixing processes in the stratosphere is a matter for speculation entirely, but it might be taken to lie between 1 and 100 days. On this basis nitrous oxide should maintain a constant mixing ratio up to 25 km and perhaps even to 40 km. Most of the photo-chemical destruction takes place below 40 km and we have assumed that the mixing ratio is constant up to this level. Taken together with the computations of Bates and Witherspoon this leads to a mean value for the total destruction rate in a vertical column of $8{\cdot}3 \times 10^{10}$ molecules $cm^{-2}$ $sec^{-1}$.* The assumption that there is nitrous oxide above 20 km might, as these authors have pointed out, possibly lead to unacceptable accumulations of nitric and higher oxides in the atmosphere. If this objection could be sustained, the estimated destruction rate might have to be decreased by a factor of three at the most, but on the existing evidence it appears to be the best estimate which can be made.

### 3. Bacterial reactions in the soil

There is little doubt that nitrous oxide does in fact form a considerable part of the nitrogen exchange between the earth and atmosphere. As far as is known, the two gases containing nitrogen which are most copiously released from nitrogenous matter by soil bacteria are molecular nitrogen and nitrous oxide (Verhoeven 1952). The proportions differ according to the conditions, but the work of Verhoeven on spore-forming bacteria acting upon nitrates in aqueous solution shows that the two gases will, on the average, be released at somewhat similar rates. Thus no real difficulty lies in the way of identifying the soil surfaces with the source of atmospheric nitrous oxide. It is desirable however to have rather more direct evidence that the rate of emission from the soil is of the right magnitude to replace the loss by photo-chemical decomposition.

Experiments carried out in this laboratory by Arnold (1953) on the loss of nitrogen from soils, involved the measurement of the nitrous oxide released to the atmosphere from a number of representative British soils under a variety of natural conditions. A feature of the results was that the rate of evolution appeared to depend critically upon the aeration of the soils. For low-aerobic conditions the greatest yields were obtained corresponding to time constants† of the order of 10 days or less. For most conditions employed the time constant was of the order of 100 days.

* It is worth noting that this rate is considerably greater than the rate at which nitrogen molecules are removed from the atmosphere as $NH_4^+$ and $NO_3^-$ ions in solution in rainwater. Angström and Höberg (1952) give an annual mean value of about 3 kg per hectare for a fairly wet climate, which corresponds to $1{\cdot}5 \times 10^{10}$ nitrogen molecules $cm^{-2}$ $sec^{-1}$.

† The time constant used by Arnold is defined by

$$\tau = \operatorname{Lt}_{t \to 0} \frac{N_s}{\frac{dN_a}{dt}},$$

where $N_s$ is the number of available nitrogen molecules in the soil in the form of $NH_4^+$ and $NO_3^-$ ions per $cm^3$ of surface, and $N_a$ is the number of nitrogen molecules in the form of nitrous oxide emitted to the atmosphere per $cm^2$ of soil surface.

Using the best available data for the extent of soil surfaces, the depth of the top-soil in equilibrium with the atmosphere, and the nitrate and ammonium ion contents of soils, it appears that a time constant of 200 days is required to supply an average over the whole globe of $8 \times 10^{10}$ nitrous oxide molecules $cm^{-2}$ $sec^{-1}$. According to the laboratory data there is no difficulty in maintaining this rate.

These calculations, details of which will be given by Arnold, demonstrate clearly that bacterial reactions could supply the nitrous oxide in the atmosphere. They are not however conclusive, and from the very nature of the data it is unlikely that improvement will make them so. A much more important aspect of the problem is the possibility of providing soil science with an estimate of the minimum size of the nitrogen cycle between the earth and atmosphere. This would be possible if it could be shown that alternative sources of nitrous oxide are insufficient to make good the photo-chemical decomposition.

## 4. Homogeneous chemical reactions

Bates and Witherspoon have examined in detail the possible mechanisms by which nitrous oxide could be produced by homogeneous chemical reactions. The most promising reaction which they considered was

$$N_2 + O_3 \rightarrow N_2O + O_2.$$

They considered that a rate coefficient of $5 \times 10^{-27}$ $cm^3$ $sec^{-1}$ is adequate to account for the nitrous oxide in the atmosphere and suggested that such a value for the coefficient might be possible.

We have been able to make an estimate of the rate of this reaction by mixing nitrogen and ozone in an absorption tube, measuring the ozone decay from the absorption in the Chappuis band and the nitrous oxide formed from the intensity in the 7·8 $\mu$ band. Radiation sources were only switched on for short periods to ensure that the molecules were principally unexcited, and a glass filter prevented radiation of shorter wavelengths than $\lambda$ 3150 from falling on to the absorption cell.

Three experiments were performed. In two, there was no certain indication of nitrous oxide, but in the third a weak doublet band of the right shape was observed at 7·8 $\mu$ from which the nitrous oxide density could be estimated to be $1{\cdot}5 \times 10^{15}$ molecules $cm^{-3}$. Assuming that only the reaction proposed by Bates and Witherspoon takes place and that there are no complications of any sort, then the results show that the rate coefficient is less than or equal to $5 \times 10^{-28}$ $cm^3$ $sec^{-1}$ at 19°C.

We believe that this simple interpretation may lead to an over-estimate of the rate coefficient. Wall reactions would presumably increase the rate of the reaction. Surface adsorption of nitrous oxide is unlikely to be of importance. Even a complete adsorbed monolayer would not affect the results in any important way, and according to the experiments of Hartley, Henry and Whytlaw-Gray (1939) the monolayer should be very far from complete at the partial pressure of nitrous oxide which was observed. Photo-chemical destruction of nitrous oxide can be neglected since the long-wave absorption edge is at $\lambda$ 3070 which is beyond the cut-off of the glass filter. Oxygen atoms can be formed when ozone molecules absorb radiation (see Dütsch 1946 for details of the various reactions and values for the rate coefficients) and these could react with nitrous oxide as follows (Henriques, Duncan and Noyes 1938)

$$N_2O + O \rightarrow N_2 + O_2 \text{ or } 2NO.$$

By estimating the maximum possible concentration of oxygen atoms in the absorption

tube, it has been possible to demonstrate that the rate of this reaction would have to be greater than $10^{-13}$ cm$^3$ sec$^{-1}$ before allowance need be made for it. According to Henriques *et al.* the minimum activation energy is 14 k cal g-mol$^{-1}$, which makes a rate coefficient of this size most unlikely.

Returning to the reaction suggested by Bates and Witherspoon for the formation of nitrous oxide in the atmosphere, let us assume its rate coefficient to be $5 \times 10^{-28}$ cm$^3$ sec$^{-1}$ at 19°C. The rate coefficient is proportional to $T^{\frac{1}{2}} \exp(-E/\mathbf{R}T)$ and hence its value at any other temperature $T$ may be deduced if the activation energy $E$ is known. An estimate has been made of the total rate of production of nitrous oxide in the atmosphere by this reaction for a series of assumed values of $E$. A simple model of atmospheric temperatures at various latitudes was taken together with ozone distributions according to recent balloon observations and, in particular, the tropospheric concentration was assumed to be $2 \times 10^{-3}$ cm S.T.P. km$^{-1}$ in accordance with Götz and Voltz (1951). The rate was found to be a minimum for an activation energy of 13 k cal g-mol$^{-1}$ and to be within a factor two of $2 \times 10^9$ molecules cm$^{-2}$ sec$^{-1}$ for activation energies between 2 and 38 k cal g-mol$^{-1}$. The actual activation energy is very probably within this range since simple kinetic theory requires a value between 20 and 30 k cal g-mol$^{-1}$ for the assumed rate coefficient.

Thus the evidence indicates that the most promising homogeneous gas reaction is too slow, by a factor of 40, to account for the observed concentration of atmospheric nitrous oxide. Needless to say, this does not dispose of the problem. In the first place our experiments must not be considered to be a satisfactory substitute for a scrupulous physical-chemical investigation of the reaction concerned. In the second place, with so many alternatives available, it is impossible to be certain that this reaction is the only one of importance. However, when faced with an alternative source which appears to be unobjectionable, we have little choice but to conclude tentatively that homogeneous gas reactions are probably of secondary importance as a source of atmospheric nitrous oxide.

## 5. Conclusion

Our conclusion does not differ greatly from that of Bates and Witherspoon, except that we prefer to reverse their emphasis. Bacterial reactions in the soil are shown to be a possible source of atmospheric nitrous oxide, while the homogeneous gas reaction which they suggest is probably of less importance. Further investigation is unlikely to influence the conclusion about the bacterial reactions, but it is highly desirable to have a more exact treatment of the gas reaction. This might be done using an apparatus similar to our own but taking greater precautions to extend the life of the ozone, and perhaps by working at higher temperatures. More precise values for the rate coefficient and the activation energy would deserve a rather detailed model of temperatures in the lower atmosphere, since, for activation energies greater than 15 k cal g-mol$^{-1}$, the reaction is confined to the lowest one or two kilometres in the tropics.

If this conclusion is correct, then it implies that a lower limit of $10^{11}$ molecules cm$^{-2}$ sec$^{-1}$ can be set to the mean rate of exchange of nitrogen between the earth and atmosphere.

## Acknowledgments

We are indebted to Dr. P. W. Arnold for permission to discuss his results prior to publication, and to Prof. D. R. Bates for a number of very helpful comments.

## References


| | | |
|---|---|---|
| Adel, A. | 1951 | *Science*, **113**, p. 624. |
| Angström, A. and Höberg. L. | 1952 | *Tellus*, **4**, Pp. 31 and 271. |
| Arnold, P. W. | 1953 | *J. Soil Sci.* In course of publication. |
| Bates, D. R. and Witherspoon, A. E. | 1952 | *Mon. Not. R. Astr. Soc.*, **112**, p. 115. |
| Dütsch, H. U. | 1946 | Dissertation, Zürich. |
| Goody, R. M. | 1953 | *The physics of the stratosphere*, Cambridge University Press (in course of publication). |
| Götz, F. P. W. and Voltz, F. | 1951 | *Z. für Naturforschung*, **6**a, p. 634. |
| Hartley, G. A. R., Henry, T. A. and Whytlaw-Gray, R. | 1939 | *Trans. Faraday Soc.*, **35**, p. 1452. |
| Henriques, F. C., Duncan, A. B. F. and Noyes, N. A. | 1938 | *J. Chem. Phys.*, **6**, p. 518. |
| Slobad, R. L. and Krogh, M. E. | 1950 | *J. Amer. Chem. Soc.*, **72**, p. 1175. |
| Verhoeven, W. | 1952 | Doctoral Thesis, Delft. |
| Yarnell, J. and Goody, R. M. | 1952 | *J. Sci. Instr.*, **29**, p. 352. |

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# On the generation of charge associated with graupel formation in thunderstorms

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Summary

It is argued that the generation and separation of electric charge above the 0°C level in thunderclouds are consequences of the growth and fall-motion of graupel pellets. The results of laboratory experiments which show the acquisition of a negative charge by a growing rime deposit are used to calculate the charge liberated during the growth, in a thunderstorm cell, of ice particles by the accretion of supercooled cloud droplets. The calculations show that, in a vigorous warm cumulonimbus, it is possible for this process to generate about 1,000 C of charge within about 11 min of the activation of ice nuclei at the − 10°C level, and that this volume of charge can be subsequently separated by the falling precipitation elements at a rate which can account for the production of several successive lightning flashes over a period of a few minutes.

## 1. Introduction

In a recent paper, Mason (1953), the author has assembled the following conditions which observations show must be satisfied by any satisfactory theory of charge generation and separation in thunderclouds – see also Wormell (1953).

(a) The average duration of precipitation and electrical activity from a single thunderstorm cell is about 30 min.

(b) The average electric moment destroyed in a lightning flash is about 110 C km, the corresponding charge being 20 C to 30 C, from which it follows that,

(c) The magnitude of the charge which is being separated immediately after a flash, by virtue of the fall-speed of the precipitation elements, is of order $\frac{8{,}000}{V}$ C, where $V$ is the fall-velocity of the particles relative to the air, in m/sec.

(d) In a large, extensive cumulonimbus this charge is generated and separated in a volume bounded by the − 5°C and the − 40°C levels and having perhaps a radius of 2 km.

(e) The negative charge is centred near the − 5°C isotherm while the main positive charge is situated some kilometres higher up; a subsidiary positive charge may also exist near the cloud base, being centred at or below the 0°C level.

(f) The charge generation and separation processes are closely associated with the development of precipitation, particularly in the form of graupel; the precipitation particles must be capable of falling through updraughts of several m/sec.

(g) Sufficient charge must be generated and separated to supply the first lightning flash within 12 min to 20 min of the appearance of precipitation particles of radar-detectable size.

In this same paper, previous theories of thunderstorm electrification are examined, as far as possible from a *quantitative* point of view, in the light of these requirements. None is found to be satisfactory. The only other mechanism suggested by laboratory experiments which appears to fit, at least qualitatively, the observed facts, is the charge generation associated with the formation of rime. In this paper we use the results of these laboratory experiments to investigate how far the liberation of charge accompanying the growth and fall-motion of graupel particles can satisfactorily account for the generation and separation of charge in a thundercloud.

## 2. Experimental evidence on the charging of a rime deposit

During recent years a number of experimenters have reported that when supercooled droplets are allowed to impinge and freeze on an ice surface, the latter acquires a substantial charge. This effect is to be distinguished from that which obtains when non-supercooled water splashes off an ice surface. In the latter case, it was shown early on by Faraday and by Sohncke that the ice became positively charged, the rebounding water drops acquiring a negative charge. The same result has been obtained recently by Gill and Alfrey (1952).

A review of some of the relevant experiments has been given recently by Wormell (1953). Briefly, Findeisen (1940, 1943) found the rime deposit acquired a *positive* charge while Kramer (1948) and Lueder (1951) found that it became *negatively* charged, the rate of charging depending on the impact velocity of the droplets. Two important researches not discussed by Wormell must be mentioned here.

Meinhold (1951) measured the electric field strength at the surface of the fuselage of an aircraft flying at 80 m $sec^{-1}$ through a supercooled Cu congestus. The deposition of rime was accompanied by a rapid rise in the field strength from about 200 V/m to 5,500 V/m, in a sense which showed that the aircraft skin was acquiring a strong negative charge. From the rate of increase of the field intensity, Meinhold calculated the rate of charging to be $5 \times 10^{-12}$ C $cm^{-2}$ $sec^{-1}$ and attributed this high rate to the high speed of impact of the droplets and their rapid freezing.

The most satisfactory and conclusive experiments have been carried out by Weickmann and aufm Kampe (1950, and private discussion*). Their cold test body was a metal rod of diameter 5 mm placed in a cold room kept at either $-5°C$ or $-12°C$. Water droplets in the diameter range 5 $\mu$ to 100 $\mu$ were sprayed on to the rod at velocities varying from 5 to 15 m/$sec^{-1}$. The droplets in the immediate neighbourhood of the rod were slightly supercooled and the water content of the cloud in this region was about 4 $gm^{-3}$. Thus, conditions relevant to the growth and fall-motion of graupel pellets or small hailstones in a cumulonimbus were fairly well simulated. Singly-distilled water, tap water, weak sodium chloride solutions and strong solutions of ammonium hydroxide and sodium fluoride were used in different experiments, but in each case the rate of charging of the rod was the same for a given air velocity. The charge generation increased with the velocity of the air stream, i.e., the rate of accretion of the rime, and for a velocity of 15 m/$sec^{-1}$ attained a value of $5 \times 10^{-12}$ C $cm^{-2}$ $sec^{-1}$. The ice deposit on the rod showed the clear and opaque layers characteristic of hailstones, but charging stopped when the ice became wet. When water at temperatures slightly above freezing was sprayed on to the rod it acquired a slight *positive* charge.

All the experiments except those of Findeisen point to the acquisition of a *negative* charge by a growing layer of rime providing it does not become wet. In the calculations to be described we shall take the figure given by Weickmann and aufm Kampe for the rate of charging as being the most reliable, particularly as it receives some independent support from the work of Meinhold.

## 3. The occurrence of graupel in thunderclouds

There is fairly conclusive evidence that the generation and separation of the charge which gives rise to the bipolar distribution in a thundercloud occurs mainly above the 0°C level. Moreover, the onset of lightning activity appears to coincide with the appearance of precipitation elements. Very valuable information on the nature of hydro-

* *Added in proof*: Word has just been received from Dr. Weickmann that the results may have been affected by electrification of the spray. Their experiments are being repeated.

meteors found in thunderstorms is provided by Kuettner (1950). His observations were made on the top of the Zugspitze (10,000 ft), which was immersed in the clouds, and were therefore more reliable and clear-cut than those made from an aircraft in which it is difficult to distinguish between snow and graupel hitting the windscreen. Kuettner reports that solid precipitation elements were predominant throughout the greater part of the cloud, being found on 93 per cent of occasions, while raindrops were present in only 21 per cent of the clouds; the cloud base temperatures were generally near 0°C. He states that graupel (in which he includes snow pellets and soft hail) was the most frequent form of hydrometeor, being present on 75 per cent of occasions and was always accompanied by strong electric fields, while large hail was relatively rare and was by no means a necessary event in a thundercloud. Snow crystals prevailed during the later periods of a storm when the precipitation became more steady and moderate in character and the lightning activity diminished.

Malan and Schonland (1951 a, b) present evidence which suggests that in South African thunderstorms the negative charge may often be distributed throughout a nearly vertical column which may extend up to, but not beyond the $-40$°C level. This is consistent with the charge being generated by growing graupel pellets because supercooled droplets may exist at temperatures down to, but not below, $-40$°C.

## 4. The growth of graupel in a cumulonimbus

The growth of ice particles by collision with supercooled droplets in a shower cloud has been discussed by Ludlam (1952). The relevant equations are

$$\frac{dr}{dt} = \frac{Ew\,(V - v)}{4\rho} \quad . \quad . \quad . \quad . \quad (1)$$

and

$$\frac{dz}{dt} = U - V \quad . \quad . \quad . \quad . \quad . \quad (2)$$

in which $r$, $\rho$, $E$ and $V$ are respectively the radius, density, collection efficiency and fall-velocity of the growing particle, $z$ its height above a fixed level, $v$ the fall-velocity of the cloud droplets, $U$ and $w$ the updraught speed and liquid-water content of the cloud, and $t$ the time.

If we assume $U$ to be constant with height and $V \gg v$, we obtain on integration of (1) and (2), the following equation for the radius of the particle as a function of its height above a given datum level $z_0$ :

$$\int_{r_0}^{r} \frac{(U - V)}{EV}\,dr = \frac{1}{4\rho}\int_{z_0}^{z} w\,dz \quad . \quad . \quad . \quad (3)$$

This equation has been used to compute the growth of ice particles by accretion in a model cloud with base temperature 10°C and pressure 900 mb, constant updraught of 10 m sec$^{-1}$ and liquid-water content as deduced from ascent along the 10°C-900 mb saturated adiabat. Growth in the early stages occurs by sublimation of water vapour but when the particle radius exceeds about 100 $\mu$ growth by accretion becomes predominant; the particles are thereafter assumed to grow at a constant density of 0·3. Typical growth curves (i.e., for particles originating at $-5$°C) are shown in Figs. 1 and 2.

When an ice particle exceeds a certain critical radius and fall-speed, the values of which depend upon the water content and the temperature, it collects supercooled droplets more rapidly than the latter can freeze, and so acquires a liquid coat. The limiting factor is the rate at which their heat of fusion can be dispersed to the environment. The heat economy of the wet hailstone has been worked out by Ludlam (1950).

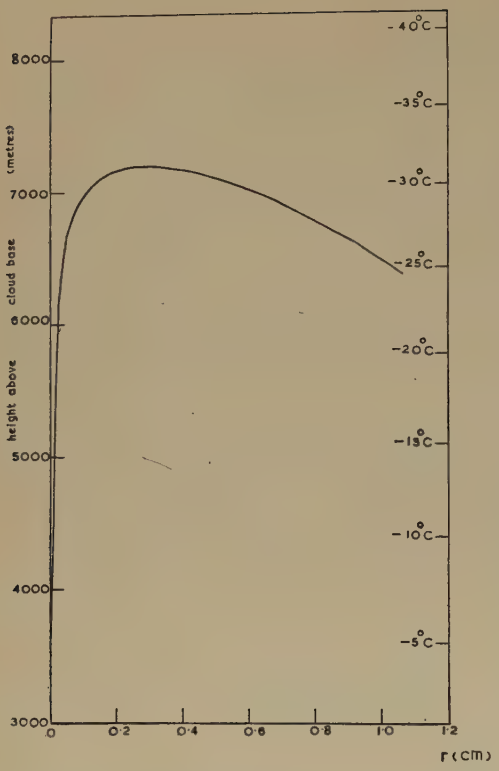


Figure 1. The radius-height curve of a graupel particle originating at the — 5°C level.

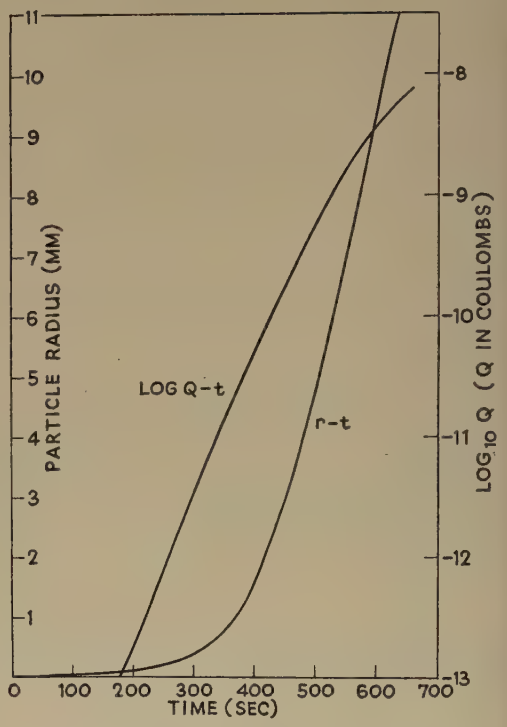


Figure 2. Radius-time and charge-time curves for a graupel particle originating at — 5°C level.

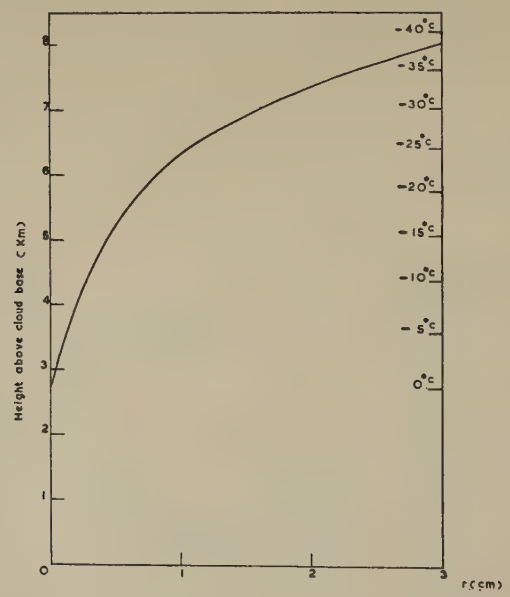


Figure 3. The maximum height at which graupel particles of given radius and a density 0·3 become wet in a cloud of base temperature 10°C and pressure 900 mb.

The maximum height at which an ice pellet of a given radius will become wet when growing in our model cloud at a density of 0·3 is shown in Fig. 3.

Having traced the growth of graupel pellets in our model cloud to the stage at which they become wet, we shall use the laboratory results on the rate of charging associated with the accretion process to calculate the charge generated during the growth of the ice particles. It will be assumed, as indicated by the laboratory experiments, that charging ceases once the particles become wet.

Calculations show (and it can be seen roughly from Fig. 1), that in our cloud, ice particles originating at temperatures below $-12°C$ will be carried beyond the $-40°C$ level with radii of only about 500 $\mu$ and so will contribute only slightly to the charge during their upward journey. In a natural cloud, the speed of the updraught probably falls off considerably at these levels, so that particles originating at temperatures below $-12°C$ may never reach the $-40°C$ level and will then be able to grow by accretion on their downward journey. Thus, in this respect, our simple cloud model will lead to an underestimate of the charge which will be generated.

Also, we shall consider the ice pellets to grow in isolation; in practice, there will be a population of particles in a given cloud volume, all competing for the available liquid water, and although we shall ignore the possible consequences of this for the time being we shall discuss them later.

We shall assume, also, the liquid-water content to attain its maximum theoretical value, i.e., no dilution of the ascending cloudy air by mixing with the drier environment. This state of affairs may not be far from the truth in the *centre* of a *large* thundercloud, but, in any case, if smaller values of $W$ are assumed, an ice particle will attain a larger radius and hence liberate more charge before becoming wet.

Remembering, then, that calculations based on the above cloud model are subject to these limitations we shall now determine whether they can predict the rate of charge production required by the observational evidence.

## 5. The generation of charge by the growth of ice particles

It is assumed that the growth of the graupel is accompanied by a constant rate of charging $\sigma = 5 \times 10^{-12}$ C cm$^{-2}$ sec$^{-1}$ as found in the experiments of Weickmann and aufm Kampe and by Meinhold. As there is some evidence that the rate of charging depends on the impact velocity of the supercooled droplets, this assumption is not strictly valid, but as the particles do not make a significant contribution to the total charge until their fall velocities exceed 15 m sec$^{-1}$ (the velocity used in Weickmann and aufm Kampe's experiment), the error involved is small.

With this simplification the rate at which charge is liberated at the surface of the particle, assumed spherical, may be written

$$\frac{dQ}{dt} = 4\pi r^2 \sigma \quad . \quad . \quad . \quad . \quad . \quad (4)$$

and the total charge $Q$ liberated by the particle up to the moment at which it acquires a liquid coat is

$$Q = 4\pi\sigma \int_0^{r_w} r^2 \, dt \quad . \quad . \quad . \quad . \quad (5)$$

where $r_w$ is the radius at which it becomes wet. The integral on the right-hand side of Eq. (5) is evaluated with the help of Eqs. (1), (2) and (3) and Fig. 3. The charge liberated by an ice pellet originating at the $-5°C$ level is shown as a function of time in curve (b) of Fig. 2. The results of other calculations are shown in Table 1.

We have seen already that the major contribution to the total charge will be made by ice particles originating between the − 5°C and − 12°C levels. To estimate the total charge generated by all the particles arising in this region of the cloud, the latter has been divided into temperature intervals of 2°C as shown in Table 1 and a figure has been assumed for the number of ice particles arising in each layer. Unfortunately, we have no reliable measurements of the concentrations of ice nuclei which become activated in clouds at these relatively high temperatures. Findeisen and Schulz (1944) have made measurements on surface air with a large expansion chamber; their estimates may be low because of sedimentation of a considerable fraction of the nuclei during their long expansion periods. Aufm Kampe and Weickmann (1951) by admitting surface air into a room-size cold chamber have found concentrations about one hundred times greater than those of Findeisen and Schulz at temperatures around − 10°C. It appears that the concentration and efficiency of ice nuclei are very variable; in the first instance we shall assume concentrations slightly larger than those given by Findeisen and Schulz but considerably smaller than those of aufm Kampe and Weickmann.

The total charge $Q_T$ generated by the particles originating at different temperatures in a thunderstorm cell of radius 2 km is shown in Table 1. It will be seen that the total charge generated by particles originating between − 4°C and − 12°C is 515 C, most of which is contributed by those starting at temperatures between − 8°C and − 12°C. This quantity of charge is adequate to meet condition (c) listed in the introduction, since the particles, while generating the major part of the charge, fall with an average speed exceeding 16 m $sec^{-1}$ and under these circumstances a charge of less than 500 C would be sufficient. The calculations show that this charge of 515 C would appear within 11½ min of the initiation of ice nuclei at the − 10°C level and within 6½ min of the visual cloud-top reaching the − 30°C level, after which glaciation may generally be observed. These times are consistent with those observed by Workman and Reynolds (1949). This mechanism is also consistent with the observation that lightning occurs only if the top of the radar echo reaches or passes the − 30°C isotherm, as reported by Workman and Reynolds (1949) and by Jones (1950).

TABLE 1. CHARGE GENERATED BY ICE PARTICLES ORIGINATING AT TEMPERATURES BETWEEN − 4°C AND − 12°C

| Temperature interval | Thickness of interval | Mean temp. | $r_w$ (cm) | Time to reach $r_w$ (sec)* | $Q \times 10^9$ (coulombs) | Concentration of particles per $m^3$ | $Q_T$ (C) |
|---|---|---|---|---|---|---|---|
| − 4°C to − 6°C | 320 m | − 5°C | 1·06 | 545 | 5·33 | $10^{-3}$ | 0·2 |
| − 6°C to − 8°C | 320 m | − 7°C | 1·16 | 595 | 6·30 | 1 | 25·4 |
| − 8°C to − 10°C | 300 m | − 9°C | 1·25 | 646 | 8·03 | 3 | 93·6 |
| − 10°C to − 12°C | 300 m | − 11°C | 1·35 | 690 | 10·5 | 10 | 395·5 |
| | | | | | | Total charge | 514·7 C |

* Calculated from time of initiation of − 10°C nuclei.

It must be pointed out, however, that according to the above calculations, the bulk of the charge is liberated only during the later stages of growth of the ice pellets, i.e., when they approach 1 cm in radius. Although soft hailstones of this size do occur they are probably not a normal feature of thunderstorms. Certainly they are not frequently observed at the ground; considerable melting of the particles may, of course, occur during their journey from the 0°C level to the ground in hot weather; under these conditions

the author has seen particles up to 5 mm in diameter mixed with raindrops. Aircraft observations of precipitation elements in the centre of an active thunderstorms are, of course, rare and in any case observations of soft hail are difficult to make because of the shattering which occurs on impact. Kuettner (1950) gives 1 cm as the maximum diameter of graupel pellets observed on the Zugspitze, true hail generally being larger than this; most of his graupel were less than 5 mm diameter. The calculations made above should perhaps be regarded as determining the *maximum* size which graupel particles could attain in the model cloud, but in many thunderclouds the particles are smaller (and more numerous) and will therefore melt more readily in falling from a warm cloud and will reach the ground predominantly as raindrops.

In the foregoing calculations we have considered a small concentration of ice particles growing without mutual interference and therefore able to attain a large size. But, the concentrations of ice nuclei in the cloud may be much higher than indicated by expansion-chamber experiments on surface air; this view receives strong support from the work of Kramer (1948), Palmer (1952) and some recent experiments by Bigg (unpublished) which indicate that the number of ice nuclei initially active in a cloud may be greatly increased by a splintering process attending the freezing of drops. A hundredfold increase in the concentrations assumed in Table 1 would compel the growing ice particle to compete for the available liquid water, with the result that they would be unable to reach the theoretical maximum size shown in Table 1. It is now important to estimate the quantity of charge which could be liberated during the growth of these smaller, more numerous graupel pellets.

Let us suppose that during the growth of the smaller particles the same mass of ice per unit volume is involved at all stages as in the above calculation but that it is divided among $N$ times as many particles. At any instant of time, therefore, the radius of the new particles will be reduced by a factor $N^{-1/3}$ and the charge per particle reduced by a factor $N^{-2/3}$; the total charge, however, will be *increased* by a factor $N^{1/3}$. If we put $N = 150$, requiring the final radii of the $-11$°C particles to be reduced from 1·35 cm to 0·25 cm, the total charge will be increased from 514·7 C as shown in Table 1 to 2,730 C. This is probably an overestimate, since the calculations are based on the assumption of maximum theoretical liquid-water content in the cloud, and indeed is a larger quantity of charge than is necessary to satisfy condition (c); the fall-velocity of the 2·5 mm particles in the upper part of the cloud is nearly 10 m sec$^{-1}$, so that only about 800 C are required. According to these last calculations, the final charge on the $-11$°C particles with $r = 2{\cdot}5$ mm is $3{\cdot}75 \times 10^{-10}$ C, which is about 4 times the maximum charge detected by Gunn (1950) on *raindrops* in a thunderstorm. This also suggests that our calculations have overestimated the charge, but the results appear to be of the right order of magnitude.

## 6. Discussion

The above theory of charge generation and separation accompanying the growth of graupel pellets appears to satisfy six of the seven conditions enumerated in the introduction and is consistent with condition (e), with modern ideas on the electrical and meteorological structure of the thunderstorm and with current theories of precipitation processes in a cumulonimbus. The calculations strongly suggest that if charge is liberated at the rate indicated by the measurements of Weickmann and aufm Kampe and of Meinhold, during the growth of graupel particles, the proposed mechanism is capable of building up a sufficient charge within 10 min or so of the activation of the first ice nuclei to supply twenty or more lightning flashes of average electric moment from a single, large thunderstorm cell.

The calculations have been made for a large single-cell type of storm where the updraught is a strong, uniform current extending up to the — 40°C level. Such a model seems to have relevance to some of the large storms studied from the electrical point of view by Malan and Schonland in South Africa. Many thunderstorms appear to have a much more complex structure with a number of smaller centres (sub-cells) of strong convective, electrical and hydrometeorological activity, corresponding perhaps to the bubbles discussed by Scorer and Ludlam (1953). In this type of cloud the distribution of precipitation elements and electric charge will be more complicated and it may be that several of these smaller centres are tapped in turn to supply the successive strokes of a lightning flash.

Kuettner (1950) and Workman and Reynolds (1950) state that a necessary condition for the development of thunderstorms on the Zugspitze and in New Mexico respectively, is that the cloud base should be warmer than 0°C. Ludlam (1951), from a study of sferics plots, comes to a similar conclusion for storms over the eastern N. Atlantic, while Scrase (1952) quotes the same condition as a distinguishing feature between the showers and thunderstorms observed at Kew.

Colder clouds possess lower liquid-water contents and, in general, weaker updraughts, so that both the growth rate and the final size of graupel particles will be smaller; the charge produced being correspondingly less. But, in view of the uncertainty in the assumptions which have to be made concerning the concentration and density of active ice particles, the water content and updraught structure of the cloud, it is difficult to draw a firm distinction between showers and thunderstorms in terms of the growth of ice particles and the attendant liberation of charge. It may be that generation of sufficient charge is not the only important condition to be satisfied for the onset of lightning and that provision of a local electric field of sufficient strength to cause breakdown of the air is often the determining factor. The establishment of a strong field may require the close proximity of positive and negative charge regions within the cloud. In this respect, the occurrence of the lower positive charge, which is a feature of cumulonimbus with base temperatures above 0°C, may be highly significant. Malan and Schonland (1951) present experimental evidence for the fact that in a high proportion of their storms the first discharge is initiated by the strong field which exists between this positive core and the base of the main negative column. It may be, then, that not only the magnitude of the charge but also its distribution is of importance in causing lightning activity.

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## References


| | | |
|---|---|---|
| aufm Kampe, H. J. and Weickmann, H. K. | 1951 | *J. Met.*, **8**, p. 283. |
| Findeisen, W. | 1940 | *Met. Z.*, **57**, p. 201. |
| | 1943 | *Ibid.*, **60**, p. 145. |
| Findeisen, W. and Schulz, G. | 1944 | *Forsch Erfahrb. Reichs. Wetterdienst* A, Berlin, No. 27. |
| Gill, E. W. B. and Alfrey, G. F. | 1952 | *Nature*, **169**, p. 203. |
| Gunn, R. | 1950 | *J. Geophys. Res.*, **55**, p. 171. |
| Jones, R. F. | 1950 | *Quart. J. R. Met. Soc.*, **76**, p. 329. |
| Kramer, C. | 1948 | *K. Ned. Met. Inst. Verh.* A, No. 54. |
| Kuettner, J. | 1950 | *J. Met.*, **7**, p. 322. |
| Ludlam, F. H. | 1950 | *Quart. J. R. Met. Soc.*, **76**, p. 52. |
| | 1951 | *Ibid.*, **77**, p. 402. |
| | 1952 | *Ibid.*, **78**, p. 543. |
| Lueder, H. | 1951 | *Z. angew. Phys.*, **3**, pp. 247, 289. |

| | | |
|---|---|---|
| Malan, D. J. and Schonland, B. F. J. | 1951a | *Proc. Roy. Soc.* A, **206**, p. 145. |
| | 1951b | *Ibid.*, **209**, p. 158. |
| | 1953 | *Ibid.*, **209**, p. 158. |
| Mason, B. J. | 1953 | *Tellus* (in the press). |
| Meinhold, H. | 1951 | *Geofis. Pur. Appl.* **19**, p. 176. |
| Palmer, H. P. | 1952 | *Air Ministry, Met. Res. Ctee., M.R.P.*, No. 702. |
| Scorer, R. S. and Ludlam, F. H. | 1953 | *Quart. J. R. Met. Soc.*, **79**, p. 94. |
| Scrase, F. J. | 1952 | *Nature*, **170**, p. 561. |
| Weickmann, H. K. and aufm Kampe, H. J. | 1950 | *J. Met.*, **7**, p. 404. |
| Workman, E. J. and Reynolds, S. E. | 1949 | *Bull. Amer. Met. Soc.*, **30**, p. 142. |
| | 1950 | *Proc. Conf. Thund. Elect.*, Chicago, p. 29. |
| Wormell, T. W. | 1953 | *Quart. J. R. Met. Soc.*, **79**, p. 3. |

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# The formation of atmospheric ice crystals by the freezing of droplets

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SUMMARY

Using the results of laboratory experiments on the supercooling of purified water, the freezing of cloud and raindrops is examined. It is shown that at temperatures lower than about — 30°C in cold-box or expansion-chamber experiments, the drops freeze in approximately the numbers that would be expected if they were pure water. Those that freeze at temperatures higher than about — 20°C seem to be more numerous than would be the case if the drops were pure. An interpretation of the main features of the Findeisen and Schulz expansion-chamber experiments is found to be possible without appealing to the action of foreign ice-forming nuclei, although there is a discrepancy between calculation and experiment at temperatures higher than about — 20°C. In the atmosphere, formation of cirrus clouds is shown to become possible at temperatures below about — 35°C, glaciation in stratiform clouds to become appreciable at temperatures below about — 20°C and freezing of raindrops in strongly convective clouds to become important below about — 13°C, without the presence of ice nuclei. It is concluded that freezing nuclei may be important at temperatures above about — 20°C, while their presence at lower temperatures will be masked by the freezing of uncontaminated drops.

## 1. INTRODUCTION

It is well known that condensation of water vapour in the atmosphere takes place on small suspended particles called ' condensation nuclei.' It has also been established that clouds composed almost entirely of water drops can exist at temperatures approaching — 40°C. On the other hand, the ice phase is often observed in clouds at considerably higher temperatures. It is generally assumed that the freezing in these cases is brought about by impurities, which in this context are called ' freezing nuclei.'

In a recent paper (Bigg 1953) the author has described some laboratory experiments on the freezing of water drops which seem, in principle, applicable to such problems of cloud physics. In order that readers may judge the validity of using these results for calculating the probability of freezing of airborne drops, the method and conclusions of that work will be briefly summarized.

A technique used by Dufour (1861) was adopted for studying the freezing of drops in the diameter range 0·1 mm to 2 cm. The drops were suspended at the interface of two insoluble liquids, one heavier and one lighter than water, and were therefore free from the constraints imposed by a solid surface. It was found that temperatures of freezing consistent within the limits of experimental error were obtained with all the combinations of liquids tried. It was also shown that results consistent with these were obtained with drops resting on a hydrophobic film of silicone oil (' drifilm ') baked on to a glass surface, and covered with an insoluble liquid; it was concluded that under these conditions the freezing of the drops was uninfluenced by their surroundings.

The numerical results obtained agreed in form with those of Dorsch and Hacker (1950) but differed quantitatively. It was found that the mean freezing temperature of drops depended upon their volume and also the rate at which they were cooled. In Fig. 1 are plotted the mean freezing points of groups of drops having different diameters, as a function of their diameters. Some more recently determined points for very small drops are included. (These small drops, produced by condensation on a drifilm surface and covered with liquid paraffin, were viewed by transmitted light with a microscope and were photographed at short intervals. The points represent a total of 314 drops

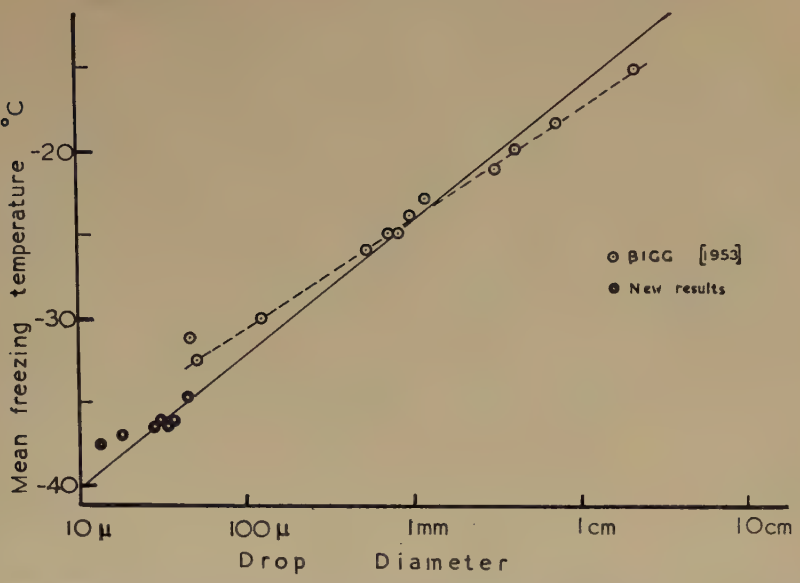



Figure 1. The mean freezing temperature of drops as a function of their diameter, when suspended at the interface of two liquids.

studied.) The full line in Fig. 1 represents a slight amendment to the values published earlier, made necessary by the new experiments; the pecked line is the one previously published. The new experiments confirm the conclusions that the probability of freezing depends upon the volume of the drop and rate of cooling. It was shown that for a drop of volume $V$ cm$^3$ cooled to a temperature $T_s$ below 0°C for $t$ seconds, the probability $P$ that it will freeze is given by

$$\ln (1 - P) = - V t K (\exp a T_s - 1) \quad . \quad . \quad . \quad (1)$$

where the factor $K (\exp a T_s - 1)$ has been empirically determined and $a$ and $K$ are constants. From the pecked line of Fig. 1, $a$ was found to be 1·0 and $K$ to be $5{\cdot}2 \times 10^{-10}$. The full line of Fig. 1 requires instead that $a$ should be 0·82 and $K$ to be $2{\cdot}9 \times 10^{-8}$. Although this may seem an excessive adjustment, the effect on mean freezing temperature is seen from the figure to be slight. As the amended results are based on more extended observations, they will be used throughout this paper.

In the case of drops being cooled, or growing, it was shown that Eq. (1) should be modified to read

$$\ln (1 - P) = - \int_0^t V K (\exp a T_s - 1) \, dt \quad . \quad . \quad . \quad (2)$$

Another conclusion was that distilled water from various sources gave the same mean freezing temperatures when cooled under the same conditions. It was supposed that the impurities present in distilled water were without effect upon freezing.

The statistical nature of freezing and the factors of volume and time have not been fully considered before in discussing the appearance of the ice phase in meteorological experiments. It is therefore proposed to apply the Eqs. (1) and (2) to the freezing of cloud and rain drops in order to assess the role played by freezing nuclei. If the point be granted that the freezing of the drops was not influenced by their environment in these experiments, the equations given above should be equally applicable to airborne drops.

## 2. An examination of experiments with chilled clouds

When a cloud is suddenly chilled to a known temperature by the passage of a cold body as in the experiments of Schaefer (1946) and Mason (1952), or by rapid expansion followed by a period of constant temperature as in the experiments of Cwilong (1947) and Fournier d'Albe (1949), we can obtain an estimate of the most probable number of ice crystals with the help of Eq. (1). The diameters of drops in these experiments were in the range of 5 $\mu$ to 20 $\mu$ and the cooling times varied from 0·1 to 1 sec. Table 1 shows the calculated fraction of droplets which will freeze as a function of the temperature and time of cooling.

TABLE 1. The mean number of ice crystals produced per 1,000 drops

| Duration of cooling | 0·1 sec | | | 1 sec | | |
|---|---|---|---|---|---|---|
| Diameter ($\mu$) | 5 | 10 | 20 | 5 | 10 | 20 |
| Temperature (°C) | | | | | | |
| − 38 | 0·0 | 0·0 | 0·0 | 0·0 | 0·0 | 0·4 |
| − 39 | 0·0 | 0·0 | 0·1 | 0·0 | 0·1 | 1 |
| − 40 | 0·0 | 0·0 | 0·2 | 0·0 | 0·3 | 2 |
| − 41 | 0·0 | 0·1 | 0·6 | 0·1 | 0·7 | 6 |
| − 42 | 0·0 | 0·2 | 1·2 | 0·2 | 1·6 | 12 |

This table shows how it is that, with a consistent method of observation, an apparently critical temperature for the formation of ice crystals can be found, for the drop-size spectrum is narrow in an artificial fog. It shows also that different experimental techniques producing droplets of different sizes and different cooling times must yield rather different critical temperatures, thus explaining why different observers have obtained values ranging from − 39°C to − 42°C. In Cwilong's and Fournier d'Albe's experiments the drops were small and their experimental volume and duration of cooling were small, which explains why their critical temperatures were the lowest reported.

In general, then, agreement between experiment and calculation is good and consequently we are justified in proceeding to estimate the freezing temperature of drops under other conditions.

## 3. An examination of the expansion-chamber experiments of Findeisen and Schulz

Various experimenters have attempted to determine the properties of freezing nuclei by allowing the air to cool by expansion and counting the number of ice crystals at various stages of the experiment. Findeisen and Schulz (1944) and Schulz (1947) came nearest to achieving atmospheric conditions by expanding rather large volumes of air slowly. Their results are well known (see e.g., Mason and Ludlam 1951) and will only be summarized here.

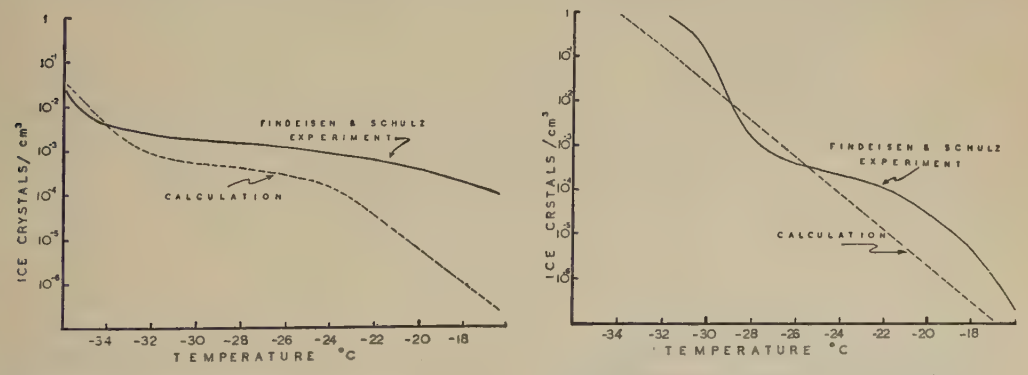
The number of ice crystals formed above − 20°C was very small and decreased with increasing rates of expansion up to a rate equivalent to an ascent speed of 35 m/sec, then increased again. The nuclei presumed responsible for the formation of these crystals were designated ' Type I.' A marked increase in the number of crystals with decreasing temperatures set in at about − 30°C and was attributed to the activation of ' Type II ' nuclei. The critical temperature at which this increase was first noted fell with increasing rates of expansion. The critical temperature of Type I nuclei depended on the expansion rate at the moment of condensation.

The expansion vessel used by Findeisen and Schulz had a depth of 2·5 m and that of Schulz 1·5 m. A little consideration shows that practically all of the original drops must have fallen out during the slowest expansions which last 20 min or so and that therefore a new population of drops must have arisen as the cooling of the air was continued. This must be taken into account in considering freezing, for, according to Eq. (1) both the volume of the drop and the time for which it has been cooled influence its probability of freezing.

The rate of precipitation of drops depends on the degree of mixing of the air in the chamber. If turbulent velocities of the order of a few cm/sec existed, then we can consider the droplets to have a uniform spatial distribution. The walls of the inner chamber were very thin but had a thermal capacity in Schulz's apparatus equivalent to about 60 l of air, and even more in the Findeisen and Schulz equipment. In Schulz's experiments, turbulence was also assured by the presence within the chamber of two 35 w lamps. It seems fairly certain that the temperature inequalities of the larger vessel would also have given rise to a certain amount of turbulence.

The method by which the number of drops present at any moment during the expansion can be calculated, assuming that the air in the chamber is kept mixed, is outlined in the appendix. It requires a knowledge of both the initial dewpoint of the air and the number of condensation nuclei. Neither of these quantities was recorded by the experimenters. At low expansion rates the freezing spectrum is found to depend quite critically on these initial assumptions. We find however that all reasonable values yield curves of a similar form, so that in order to illustrate the method of calculation, we shall arbitrarily choose a dewpoint of 7·5°C and 1,000 activated nuclei/cm$^3$. One other important assumption has to be made. As the expansion progresses and the original drops fall out, the supersaturation will commence to rise. At what point will a new set of nuclei be activated ? An arbitrary decision must be made here, for in practice the supersaturation will be prevented from rising at the maximum rate by the precipitated ice and water in the chamber. In order to calculate Fig. 2 we have assumed that when the number of drops falls to 1/cm$^3$, new ones commence to be generated at such a rate that the liquid-water content remains constant.

The calculated curve for 5 m/sec in Fig. 2 (a) suggests that the rather sudden rise in the number of ice particles observed at temperatures below — 30°C may be due merely to the physical limitations of the apparatus rather than the appearance of a new type of ice nucleus. Fig. 2 (b) shows the 20 m/sec curve, again compared with the Findeisen and Schulz results. In this case calculation and experiment are in closer agreement.




(a) Expansion rate equivalent to an ascent of 5 m/sec. (b) expansion rate equivalent to an ascent of 20 m/sec.

Figure 2. Comparison of the number of ice crystals found by Findeisen and Schulz, with the number calculated from the freezing of the droplets.

The initial trend of the experimental observations with increasing expansion rates can be explained. The bend in the curve moves initially upwards and to the left, causing a fall in the temperature at which, say, 100 crystals/litre is observed. This was mentioned by Findeisen and Schulz in passing, but not recorded on their diagrams. At higher expansion rates the bend disappears altogether, so that 100 crystals/litre would be observed at a considerably higher temperature.

The lower end of the curve moves to the left with increasing expansion rates, because the faster rate of cooling reduces the probability of freezing. The reversal of this trend at higher expansion rates found by Schulz cannot be matched by calculation without introducing dubious assumptions.

We conclude from this examination that the main features of the experiments can be explained without invoking ice nuclei at all. The reasonable agreement with the 20 m/sec curves makes it seem likely that the difference between calculation and experiment is largely a function of the experimental design and measurement.

We must not infer from this that freezing nuclei do not exist in the atmosphere but only that they are of minor importance in these rather artificial laboratory conditions.

There have been many cold-box or expansion-chamber experiments in recent years, but in each case there is a lack of knowledge of the conditions which prevents an accurate comparison by calculation. As examples we could mention the experiments of Palmer (1949), expansion chamber; Reynolds (1950), cold box; aufm Kampe and Weickmann (1951), cold room. The number of ice crystals reported in each of these experiments was greater at temperatures higher than $-20°C$ than can reasonably be explained by our method of calculation.

The chief lesson to be learnt from this analysis is that such experiments are subject to grave misinterpretation unless the importance of the history of the drops and the physical dimensions of the apparatus is realized.

## 4. Ice formation in clouds

Freezing of droplets in clouds can be predicted in the same way as for the laboratory experiments just described. Reliable observational checks are few because of the difficulty of distinguishing from an aircraft between supercooled drops and crystals. Temperature measurements in clouds are also somewhat unreliable. Moreover, the development of a fibrous appearance has often been taken to imply the existence of ice crystals, but, as Ludlam (1952) has pointed out, it may in certain circumstances be due to large water drops.

Again, we must bear in mind the possibility that there is secondary generation of ice particles in the atmosphere. It has often been suggested that splinters could be detached from growing ice crystals and if this should happen the number of ice particles actually occurring may far exceed the number calculated on the basis of the freezing of droplets.

### *Cirrus*

Cirrus clouds consist entirely of ice particles except perhaps during their formation and early growth. If in a growing cloud 1 per cent of the droplets freeze within about a minute of their formation, then the crystals may be expected to grow rapidly and cause the evaporation of the droplets, so that a fibrous-cirrus ice cloud with no ' mother cloud ' develops. The temperature at which this is likely to occur can be estimated from Table 2.

TABLE 2. THE MEAN TIME (SEC) TAKEN FOR 1% OF DROPS TO FREEZE

| Diameter ($\mu$) | 0·5 | 5 | 50 |
|---|---|---|---|
| Temperature (°C) | | | |
| − 30 | $1{\cdot}6 \times 10^{8}$ | $1{\cdot}6 \times 10^{5}$ | 160 |
| − 35 | $2{\cdot}8 \times 10^{6}$ | $2{\cdot}8 \times 10^{3}$ | 2·8 |
| − 40 | $4{\cdot}8 \times 10^{4}$ | 48 | 0·05 |
| − 45 | 900 | 0·9 | $9 \times 10^{-4}$ |

Since at high levels condensation nuclei are scarce, the droplets rapidly acquire diameters exceeding 5 $\mu$, and we therefore see that this kind of cirrus cloud is unlikely to develop unless the temperature is below − 30°C, but could always appear at heights where the temperature approaches − 40°C. This is in broad agreement with the results of Weickmann (1949), who found that a frequency distribution of cirrus tops as a function of temperature showed a maximum in the range − 46°C to − 50°C, with occasional cases occurring at temperatures as high as − 30°C.

### *Stratus*

In a moderately thick layer of stratus cloud drops may have very long lives at approximately constant temperatures. If we assume the drops to be of equal size, then we can write the volume of each drop as $V = w/N$ where $w$ is the liquid-water content and $N$ the number of drops per unit volume. To find the temperature at which there will, on the average, be one ice crystal per unit volume, if the probability of freezing is very much less than unity (so that $\ln(1 - P) \simeq P$), we can write from Eq. (1)

$$1 = NP = 2{\cdot}9 \times 10^{-8}\,(e^{0{\cdot}82\,T_s} - 1)\,wt$$

and obtain Table 3. This table shows that we should not expect ice crystals in concentrations greater than 1/litre unless the temperature falls below about − 20°C. Mason and Howorth (1952) in an analysis of precipitation from stratiform clouds over Northern Ireland state that, 'there was a marked increase in the frequency of rain/snow when the cloud-top temperature fell below about − 12°C, suggesting that only below this temperature does the concentration of ice-forming nuclei, on the average, become significant for precipitation release.' No clouds which had summit temperatures higher than − 13°C gave snow. In fact, in only 3 out of the 10 well-substantiated cases of snow was the summit temperature higher than − 29°C.

TABLE 3. THE TEMPERATURES AT WHICH AN AVERAGE OF ONE ICE CRYSTAL WILL FORM BY DROP FREEZING IN VARIOUS VOLUMES AND TIMES IN CLOUDS OF DIFFERENT LIQUID-WATER CONTENTS

| Time | Liquid-water content $g\ m^{-3}$ | Volume $cm^3 \times 10^6$ | $cm^3 \times 10^3$ | $cm^3$ |
|---|---|---|---|---|
| 1 sec | 0·1 | − 24°C | − 32°C | − 41°C |
| 1 sec | 1·0 | − 21°C | − 29°C | − 38°C |
| 1 min | 0·1 | − 19°C | − 27°C | − 36°C |
| 1 min | 1·0 | − 16°C | − 24°C | − 32°C |
| 1 hr | 0·1 | − 14°C | − 22°C | − 31°C |
| 1 hr | 1·0 | − 11°C | − 19°C | − 28°C |

Isolated cases have been reported of ground fogs and low stratus which contained ice crystals when their minimum temperatures were above — 10°C. Findeisen and Schulz report one such occasion on which ice crystals were seen at — 6°C. Dessens (1950) reports that all clouds below — 12°C observed at the summit of Puy de Dôme contained ice crystals. This was not, apparently, the temperature of the cloud top and he also remarked that measurements at mountain summits must often be treated with reserve because of the snow-covered ground in the neighbourhood.

### *Cumulus*

Conditions in cumulus are not as steady as in stratus, but the number of cloud droplets freezing in a cumulus will be comparable with those given in Table 3. Probably an important source of ice particles in cumulus clouds (and possibly also in stratus clouds) is the freezing of rain or drizzle drops, which are larger than the representative cloud elements. It is now well established that these larger drops can be sufficiently numerous to provide rain without the intervention of the ice phase; Bowen (1950) and Ludlam (1951) have considered their growth by coalescence in clouds having various updraughts and water contents. As far as freezing is concerned, the most important period is that during which the drop is suspended in the updraught near the cloud summit. Assuming that it spends 100 sec near the summit, we derive Table 4.

TABLE 4. THE TEMPERATURES AT WHICH DROPS OF DIFFERENT DIAMETERS MUST BE KEPT FOR 100 SEC TO HAVE PROBABILITIES OF FREEZING OF 0·1% AND 10%

| Diameter of drops | Probability of freezing | |
|---|---|---|
| mm | 0·1% | 10% |
| 0·1 | — 28°C | — 32°C |
| 0·3 | — 23°C | — 29°C |
| 1·0 | — 19°C | — 25°C |
| 3·0 | — 15°C | — 21°C |
| 5·0 | — 13°C | — 19°C |

The largest drops found in clouds would therefore commence to freeze in significant quantities at about — 13°C. The copious spicules thrown out by large drops when they freeze could become an important secondary source of ice particles.

Ludlam (1952) summarizes soundings made over the North Atlantic by the British Meteorological Flights during the period Jan. 1944 to Mar. 1946 and also observations on summer cumulus over New Mexico by Braham, Reynolds and Harrell (1951), in a diagram in which the percentage of convective clouds reported as cumulonimbus is plotted as a function of their summit temperature. This reveals a considerable difference between winter and summer clouds, the percentage of cumulonimbus being much smaller, for a given temperature, in winter than in summer. This could be at least partly due to the smaller number of drops freezing in the winter clouds of lower water content. The observations seem to be generally compatible with Table 4 with the exception of the small percentage of cumulonimbus occurring with summit temperatures above — 10°C.

It must be emphasized again, however, that a convective cloud may contain ice without appearing to be a cumulonimbus, while a cumulonimbus does not necessarily contain the ice phase.

More definite evidence of freezing in relatively warm clouds has been given by Coons, Jones and Gunn (1949) in describing traverses through cumulus of the Gulf Coast of the United States. They state that ' usually when the temperature at the tops of such clouds was below $-6°C$, natural ice crystals were detected during in-cloud traverses.'

Bowen (1952), too, has reported that hail fell from clouds with summits at $-6°C$ and $-7°C$ when sprayed with water. Even allowing copious secondary generation of ice crystals from a frozen drop, these temperatures are far too high to be explained by the freezing of purified water.

## 5. Discussion

Now that we have discussed quantitatively the effects to be expected from the freezing of distilled water, we must ask the question : are freezing nuclei really important in atmospheric phenomena ?

In the cold-box experiments at about $-40°C$ there seemed to be reasonable agreement of calculation with experiment. We therefore do not need to postulate the presence of nuclei, for homogeneous nucleation for example, as proposed by Mason, would serve equally well to explain freezing.

The experimental observations of Findeisen and Schulz made with expansion rates of 5 m/sec showed a growing departure of calculation from experiment at higher temperatures, amounting to as much as 5°C for corresponding crystal counts. Is this discrepancy genuine ? It cannot be completely explained by making different initial assumptions. Yet, if it is real, it should persist at other expansion rates. Now, we have seen that at 20 m/sec, a rate of ascent for which the physical dimensions of the vessel and the initial conditions are much less important than at 5 m/sec, the difference is much smaller. This casts doubt upon whether freezing nuclei are really responsible for the difference between observation and calculation at slow rates of expansion.

Observations of naturally-occurring clouds have, however, provided us with some examples where ice crystals appear to have been present in far greater quantities than would be expected if they were formed by the freezing of purified water. These cases are those where there is possibility of industrial smoke contaminating the drops (fogs and low stratus), or where strong convection can draw up large supplies of foreign particles from the ground. There is laboratory evidence, of course, that soil particles and combustion products can produce ice crystals in a supercooled cloud.

It seems fair to say that in many well-supercooled clouds the rate of transformation from water to ice may take place as though the water were pure, but can on occasions be speeded up by contamination with impurities. We might perhaps have expected this result, for we know that there are some special substances, such as silver iodide, which can cause cloud droplets to freeze at temperatures as high as $-5°C$.

## 6. Conclusion

This examination indicates that freezing nuclei may exercise a far less widespread influence than has previously been thought. It shows that expansion-chamber experiments need very careful interpretation and that in them, measurements should be made of dewpoint, droplet size and concentration, as well as of the number of ice crystals.

It shows that ice nuclei are important on occasions in natural clouds. It shows too, the need for statistical data on the frequency of occurrence of ice crystals in such clouds and emphasizes the need for observing the history of the cloud as well as its water content, if deductions are to be made about the efficiency of ice nuclei in promoting freezing.

## Acknowledgments

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## References

| | | |
|---|---|---|
| aufm Kampe, H. J. and Weickmann, H. | 1951 | *J. Met.*, **8**, p. 368. |
| Bigg, E. K. | 1953 | *Proc. Phys. Soc.* B, **66**, p. 688. |
| Bowen, E. G. | 1950 | *Austr. J. Sci. Res.* A, **3**, p. 193. |
| | 1952 | *Quart. J. R. Met. Soc.*, **78**, pp. 335, 337. |
| Braham, R. R., Reynolds, S. E. and Harrell, J. H., Jr. | 1951 | *J. Met.*, **8**, p. 416. |
| Coons, R. D., Jones, E. L. and Gunn, R. | 1949 | *U.S.W.B. Res. Pap.*, No. 33. |
| Cwilong, B. M. | 1947 | *Proc. Roy. Soc.* A, **190**, p. 137. |
| Dessens, H. | 1950 | *Cent. Proc. R. Met. Soc.*, p. 59. |
| Dorsch, R. G. and Hacker, P. | 1950 | *N.A.C.A. Tech. Notes*, No. 2142. |
| Dufour, L. | 1861 | *Arch. Sci. Phys. Nat.*, **10**, p. 364. |
| Findeisen, W. and Schulz, G. | 1944 | *Forsch. Erfahrb. Reichs. Wetterdienst*, Berlin A, No. 27. |
| Fournier d'Albe, E. M. | 1949 | *Quart. J. R. Met. Soc.*, **75**, p. 1. |
| Ludlam, F. H. | 1951 | *Quart. J. R. Met. Soc.*, **77**, p. 402. |
| | 1952 | *Ibid.*, **78**, p. 543. |
| Mason, B. J. | 1952 | *Ibid.*, **78**, p. 22. |
| Mason, B. J. and Howorth, B. P. | 1952 | *Ibid.*, **78**, p. 226. |
| Mason, B. J. and Ludlam, F. H. | 1951 | *Phys. Soc. Rep. Progr.*, XIV, p. 147. |
| Palmer, H. P. | 1949 | *Quart. J. R. Met. Soc.*, **75**, p. 17. |
| Reynolds, S. E. | 1950 | *New Mexico School Mines Res. Rep.*, No. 6. |
| Schaefer, V. J. | 1946 | *Science*, **104**, p. 457. |
| Schulz, G. | 1947 | *Ber. Deutsch Wetterdienst U.S. Zone, Bad Kissingen*, No. 1. |
| Weickmann, H. | 1949 | *Ibid.*, No. 6. |

## Appendix

### Outline of calculation for Findeisen and Schulz experiment

First we must calculate the number of drops present and their volume at any time. Then the probability of their freezing can be found from Eq. (2) in Section 2.

For the first step we assume the drops to be uniformly distributed by turbulent mixing, so that the rate of decrease in the population is given by

$$\frac{dN}{dt} = -\frac{Nv}{z} \qquad . \quad . \quad . \quad . \quad (3)$$

where $N$ is the number of drops/cm$^3$, $v$ their fall-velocity relative to the air, and $z$ is the total depth of the vessel. The drops are assumed to be all of equal size.

Then $v \simeq 1{\cdot}3 \times 10^6\, r^2$ where $r$ = radius (cm). Their volume $V = \frac{4}{3}\pi r^3 = (w - w_p)/N$ where $w$ is the total liquid-water content/cm$^3$ if none had been precipitated and $w_p$ the amount precipitated from each cm$^3$.

Thus, $$v = 1{\cdot}3 \times 10^6 \{3\,(w - w_p)/4\pi N\}^{2/3}$$

Substituting in Eq. (3), $$-dN/N^{1/3} = \frac{1{\cdot}3 \times 10^6}{z}(\tfrac{3}{4}\pi)^{2/3}\,(w - w_p)^{2/3}\,dt$$

Integrating, $$N_0^{2/3} - N^{2/3} = \frac{2{\cdot}6}{3} \times \frac{10^6}{z}(\tfrac{3}{4}\pi)^{2/3} \int_0^t (w - w_p)^{2/3}\,dt$$

The integral can be evaluated in small steps, taking $w_p$ at its value for the preceding step and the new $w_p$ found.

Assuming $N = 1{,}000$, $T = 7{\cdot}5°\text{C}$ when $t = 0$, $z = 2{\cdot}5$ m, the time at which $N$ has fallen to $1/\text{cm}^3$ can be found. It is then assumed that new drops are generated in sufficient numbers to maintain a constant liquid-water content.

Having found the number of drops, their volume and their temperature as a function of time, the probability of their freezing can be calculated from the integral of Eq. (2).

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# The length of dry and wet periods

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SUMMARY

A study was made of the length of dry and wet spells at five Canadian cities. From the results of the study it was learned that after a wet day the probability of the following day being wet is constant no matter how long the wet period has persisted; the same is true for the weather following a dry day except that there is a slight increase in the probability of dry weather with increasing length of the dry period.

As a corollary to the study, the probability of the weather following either a wet or dry day was determined for each month for each of the five cities. Also a simpler method is suggested for determining approximations to these figures.

## 1. INTRODUCTION

In the opinion of the general public, the probability of a type of weather continuing increases with the increasing length of the period. For instance, people believe that after a long period of dry weather it is hard for rain to come. There have been studies of spells of weather in meteorological literature, some of which are referred to below. This present study treats the subject more fully than any discovered by the author. It reaches the conclusion that the facts do not support the general opinion or only to an extent that could not be observed by the general public.

## 2. MONTREAL DRY AND WET PERIODS

A count was made of the periods of dry weather and wet weather at Montreal, Quebec. A day was considered dry if no measurable precipitation fell. With measurable precipitation, the day was counted wet. The length of a dry or wet period was the number of consecutive dry or wet days, respectively. The periods were listed by months. When a period overlapped the end of the month, it was allotted to the month in which the first day occurred. The period studied began in 1874, and the data were used to December 1951.

TABLE 1. MARCH DRY PERIODS IN MONTREAL

| Days ($n$) | Dry periods lasting $n$ days | Dry periods lasting $n$ days or longer ($y$) | Departure of log $y$ from trend line |
|---|---|---|---|
| 1 | 192 | 496 | + 0·06 |
| 2 | 117 | 304 | + 0·02 |
| 3 | 75 | 187 | 0·00 |
| 4 | 41 | 112 | − 0·03 |
| 5 | 26 | 71 | − 0·04 |
| 6 | 17 | 45 | − 0·06 |
| 7 | 6 | 28 | − 0·07 |
| 8 | 6 | 22 | + 0·01 |
| 9 | 5 | 16 | + 0·05 |
| 10 | 6 | 11 | + 0·08 |
| 11 | 0 | 5 | − 0·07 |
| 12 | 2 | 5 | + 0·11 |
| 13 | 0 | 3 | |
| 14 | 1 | 3 | |
| 15 | 0 | 2 | |
| 16 | 0 | 2 | |
| 17 | 1 | 2 | |
| 18 | 1 | 1 | |

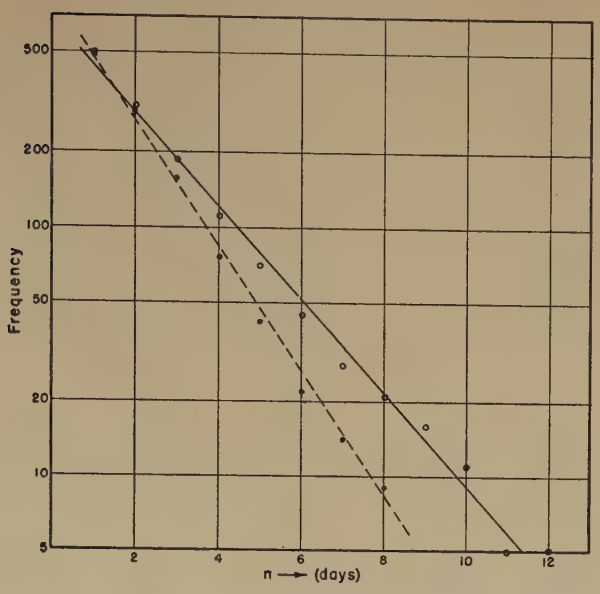


Figure 1. Number of dry (O) and wet (●) periods at Montreal equal to or exceeding a given number of days.

Table 1, column 2, gives the frequency of dry spells of different lengths during March. As was to be expected, the frequency of dry spells decreased rapidly as $n$, the number of days, increased, until $n$ reached 7 days, and the frequency became 6. Thereafter the frequency was irregular, with the longest dry spell 18 days.

A much more significant set of figures can be obtained by forming the cumulative totals from large $n$'s to small. These are given in column 3 of Table 1, and reveal the number of times a dry period $n$ days or longer occurred over the period of observation. According to the table, there were 304 dry periods of 2-day duration, 187 of which lasted a third day. The probability that after 2 days of dry weather the next day will be dry is then 187/304 = 62 per cent.

Further information is given by plotting these values until $n = 12$ on semi-logarithmic paper, as shown by the circles in Fig. 1. Sampling errors for values beyond $n = 12$, when the cumulative total $y = 5$, cause them to have very little significance. As plotted, the linear relationship becomes apparent. The equation of the line of least squares fitted to the data is

$$\log y = 2{\cdot}835 - 0{\cdot}187\, n \quad . \quad . \quad . \quad . \quad (1)$$

The base for logarithms is 10 in this equation and throughout the paper.

This line is drawn in Fig. 1. A comparison with the plotted points indicates the goodness of fit. The same information is given in column 4 of Table 1 which gives the difference between the value of $\log y$, and that given by Eq. (1). These are relatively large for $n$ greater than 9 days where the sampling errors are large. In comparison with $\log y$, for which the values drop from 2·70 to 0·70, with increasing $n$, the departures are small. The departures are negative between $n = 4$ and $n = 7$, and positive outside these limits. Part of the explanation of this trend of departure lies in the fact that the data for $y$ are cumulative, and so there is a definite relationship between one point and its neighbours.

Table 2 gives similar data for the March wet periods at Montreal. The data from Table 2, for $n \leqslant 8$, are plotted by dots in Fig. 1. The trend line

$$\log y = 2{\cdot}948 - 0{\cdot}258\, n \quad . \quad . \quad . \quad . \quad (2)$$

is drawn by a broken line.

TABLE 2. MARCH WET PERIODS IN MONTREAL

| Days ($n$) | Wet periods lasting $n$ days | Wet periods lasting $n$ days or longer ($y$) | Departure of log $y$ from trend line |
|---|---|---|---|
| 1 | 196 | 492 | 0·00 |
| 2 | 139 | 296 | + 0·04 |
| 3 | 81 | 157 | + 0·03 |
| 4 | 34 | 76 | − 0·04 |
| 5 | 20 | 42 | − 0·04 |
| 6 | 8 | 22 | − 0·06 |
| 7 | 5 | 14 | + 0·01 |
| 8 | 5 | 9 | + 0·07 |
| 9 | 3 | 4 | |
| 10 | 0 | 1 | |
| 11 | 0 | 1 | |
| 12 | 0 | 1 | |
| 13 | 0 | 1 | |
| 14 | 0 | 1 | |
| 15 | 1 | 1 | |

The facts which were discovered with the dry periods are apparent for the wet periods. The straight-line relationship is a very good fit, at least for a first approximation, to the frequency of dry spells or wet spells lasting a given number of days or longer. At least in the two examples shown, the departures from the linear relationship are positive for small $n$, negative in the middle range, and irregular but comparatively large for large $n$. Once again these departures are very small in comparison with the values of log $y$.

Eqs. (1) and (2) can be transformed into Eqs. (3) and (4) respectively. For dry periods

$$y = 680 \times (0{\cdot}650)^n \quad . \quad . \quad . \quad . \quad (3)$$

For wet periods,

$$y = 887 \times (0{\cdot}552)^n \quad . \quad . \quad . \quad . \quad (4)$$

The equations are useful in determining probabilities. If a dry period has lasted $n_1$ days, the probability that it will last $n_1 + 1$ days is

$$\frac{y_{n_1+1}}{y_{n_1}} = 0{\cdot}650$$

This quantity is independent of $n$. In other words, if one day is fine, the probability that the following day will be fine is constant, no matter how long the dry spell has lasted. This statement holds only to the extent that the linear relationship for log $y$ holds.

A semi-independent check on the probability given above can be obtained from the original data. The total number of dry days was 1,315 days. There were 492 wet spells, each of which must have been preceded by a dry day. The probability of a wet day following a dry day is then, 492/1,315 = 0·37. The two values, 0·37 and 0·65, add to 1·02, whose difference from unity is well within the limits of error of the data. A similar check with the original data gives the probability of a dry day following a day with precipitation as 0·44.

### 3. CRITICISM OF THE LINEAR RELATIONSHIP

The preceding discussion is based upon the hypothesis that the relationship between log $y$ and $n$ is linear. A further investigation on this subject is desirable. Are the departures as shown in Fig. 1 the results of the errors of sampling? Or are the data for March typical of other similar results?

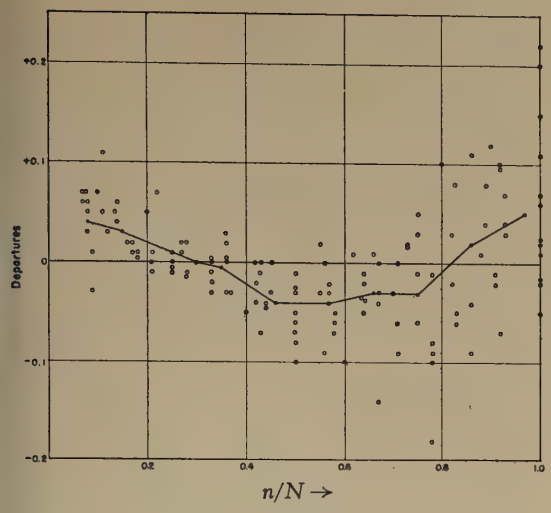


Figure 2. Departures of log $y$ from trend lines, dry periods in Montreal.

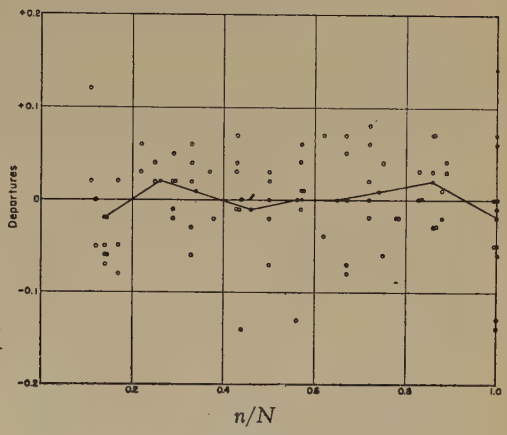


Figure 3. Departures of log $y$ from trend lines, wet periods in Montreal.

Computations similar to that given for March were done for dry and wet periods for each of the months of the year. The departures of log $y$ from the computed trend lines for each of the months are plotted in Figs. 2 and 3. Fig. 2 gives the departures for the 12 series of dry periods, and Fig. 3 for the wet periods. The abscissae used in these figures are $n/N$, where $N$ is the largest number of days used in computing the trend line. Thus for dry periods in March, $N = 12$.

A visual examination of the two figures shows that the trend of the departures noted for March data is typical of other series. For dry periods, the departures tend to be positive at both ends of the trend line and negative for the middle section. The departures for wet periods show very little evidence of any trend. Lines drawn on the two figures bring out the difference clearly. The lines join the points of mean values, when the data are grouped into classes with $n/N$ having values : 0-0·10; 0·11-0·20; 0·21-0·30; etc.

Fig. 3 gives excellent support to the hypothesis that the probability of a day being wet, provided the preceding day is wet, remains constant, no matter how long the wet period has persisted. The hypothesis is not so well supported for dry periods. There is a tendency for the probability to increase gradually as the dry period lengthens.

Although there is a trend in the departures of log $y$ from the straight line, the values of the departures are in general small when compared to actual values of log $y$. This latter is limited by the method of computation to the minimum value of 0·70. If one were to amend the slopes of the lines,

$$\log y = a + bn$$

to allow for slope of the line in one section of Fig. 2, the resulting equation would give a different value of the probability. Yet since the departures are so small, the resulting probabilities would differ by less than 5 per cent from those given by the original equations. As a very good first approximation, the probability of continued dry weather remains constant no matter how long the period has continued.

## 4. Analysis for other Canadian cities

The preceding analysis deals with the weather at Montreal. It leads one to ask whether the conclusions are valid only at Montreal and in places where the weather is similar. Or are the same conclusions valid in other climatological regions ?

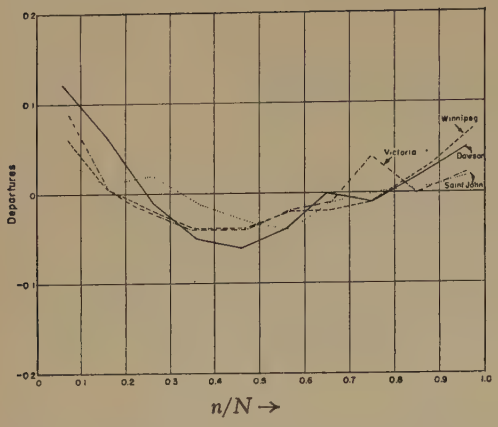


Figure 4. Mean departures from trend lines, dry periods.

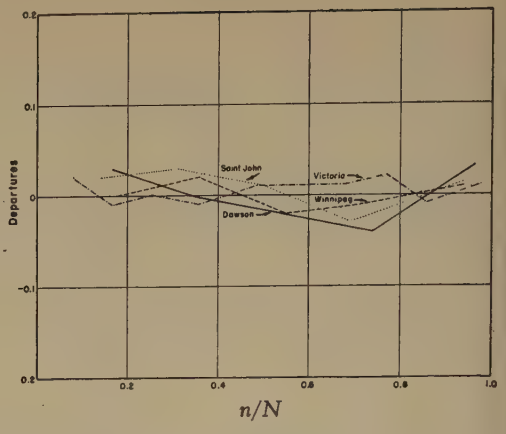


Figure 5. Mean departures from trend lines, wet periods.

To consider this problem, records were collected from other Canadian cities and similar studies were made. Those chosen were : Saint John, New Brunswick, in the uniformly moist eastern maritime region; Winnipeg, Manitoba, in the dry prairie provinces; Dawson, Yukon, in the Arctic region; and Victoria, British Columbia, where wet winters follow dry summers. The number of years of records used were : at Saint John, 69 years; Winnipeg, 76 years; Dawson, 50 years; Victoria, 52 years.

Fig. 4 presents the mean departures from the trend line for the dry periods for each of the four cities. They were computed in the same manner as the line given in Fig. 2. Fig. 5 does the same for the wet periods, except that the class intervals for $n/N$ in some instances are 0·0-0·20; 0·21-0·40; 0·41-0·60; etc.

Figs. 4 and 5 show that the mean departures from the trend lines are similar for these other cities to those for Montreal. From the results of these, one can reasonably conclude that the same law holds concerning the lengths of dry periods and wet periods no matter under what climatic regime the place lies. For wet weather, the law states that after a wet day the probability that the following day is wet is constant, no matter how long the wet period has lasted. During dry weather, the probability that the following day is dry increases slightly as the dry period lengthens.

A series with a large number of long dry periods was found for February at Dawson. There were 201 dry periods, of which 5 lasted 30 days or longer. A parabola of the form

$$\log y = a + bx + cx^2$$

as well as the straight line, was fitted to the 30 items by the method of least squares. The equation was

$$\log y = 2{\cdot}393 - 0{\cdot}1088\, n + 0{\cdot}001874\, n^2$$

This gave a better fit, with a mean departure of ·0027, in comparison with a mean departure of 0·0113 from the straight line. But for $n = 29{\cdot}0$ days, the slope of the parabola is zero; for larger $n$, $\log y$ increases as $n$ increases, an impossible result. It is possible that the various series for dry periods might be fitted better by some curve other than the straight line used in the above studies. However, as seen in Figs. 2 and 4, the departures are not large. Also the mean value of the probability as determined by the trend line is not far from the true value during any period of dry weather.

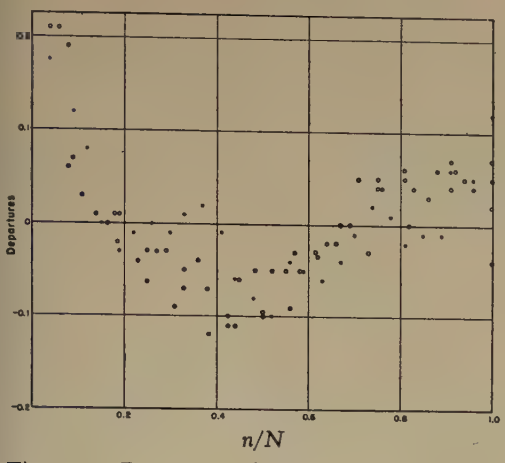


Figure 6. Departures of log $y$ from trend lines. Dry periods in the United Kingdom (O), and San Francisco (●).

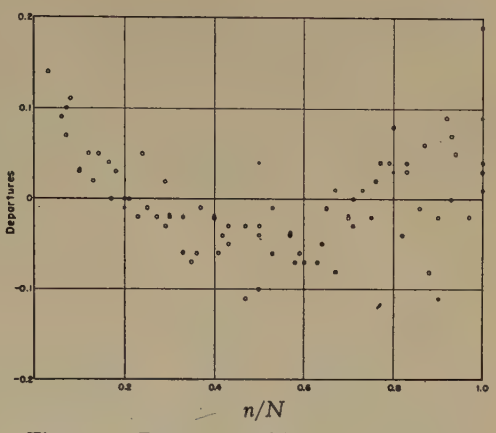


Figure 7. Departures of log $y$ from trend lines. Wet periods in the United Kingdom (O), and San Francisco (●).

## 5. Dry and wet periods, United Kingdom and San Francisco

Newnham (1916) gave data on the number of dry and wet spells of various lengths for Kew, Greenwich, Aberdeen, and Valencia. These data were treated in the same manner as the data for Montreal. The ' least-squares ' lines were :

| | Dry<br>log $y$ | Wet<br>log $y$ |
|---|---|---|
| Kew | $2{\cdot}864 - 0{\cdot}127\,n$ | $2{\cdot}904 - 0{\cdot}178\,n$ |
| Greenwich | $3{\cdot}115 - 0{\cdot}115\,n$ | $3{\cdot}336 - 0{\cdot}185\,n$ |
| Aberdeen | $2{\cdot}901 - 0{\cdot}199\,n$ | $2{\cdot}855 - 0{\cdot}133\,n$ |
| Valencia | $2{\cdot}609 - 0{\cdot}161\,n$ | $2{\cdot}565 - 0{\cdot}061\,n$ |

The departures of the data from these straight lines are given by the circles in Figs. 6 and 7, where Fig. 6 gives the departures for the dry periods and Fig. 7 for the wet periods. The trend of the departures noted for dry periods of Montreal is more apparent in Figs. 6 and 7 than in Figs. 2-5. One would conclude that the probability of the type of weather continuing increases in the United Kingdom as the period lengthens, whether it is a dry or a wet period. This was the conclusion reached by Newnham in his discussion of the data.

When Newnham obtained his data, he did not differentiate between the months in which the periods fell. Rather he included all periods during the years which he studied. According to our work with Canadian data, there is a variation in probabilities during the year. A failure to note this possibility explains some of the departures from a straight-line trend.

Assume that there are two series, given by the equations

$$y_1 = A_1\,p_1{}^n \quad \text{and} \quad y_2 = A_2\,p_2{}^n$$

For simplicity, assume $A_1 = A_2$. If the two series are combined

$$y_1 + y_2 = A_1\,(p_1{}^n + p_2{}^n)$$

When $n = n_1$ the probability that the next day will extend the period is given by

$$\frac{(y_1 + y_2)_{n_1+1}}{(y_1 + y_2)_{n_1}} = p_1 - \frac{p_1 - p_2}{1 + (p_1/p_2)^{n_1}}$$

If $p_1$ and $p_2$ are not equal, but $p_1 > p_2$, the fraction decreases in value and the probability approaches $p_1$ as $n$ increases. Thus the combination of two series, each of which fits a

straight line on semi-logarithmic paper does not result in a new series which has a linear relationship. Thus the conclusion reached by Newnham does not rule out the possibility that the data for each month would fit the curve $y = A\,p^n$.

Jorgensen (1949) gave a table giving similar information on wet and dry spells at San Francisco. He gave the number of dry and wet periods of various lengths during the months of December-March for the twenty years 1927-47.

The data were fitted by the curves :

$$y = 257\,(0{\cdot}856)^n \quad \text{for dry periods}$$

and

$$y = 450\,(0{\cdot}637)^n \quad \text{for wet periods}$$

These curves were a very good fit to the original data. The departures are given by the dots in Figs. 6 and 7. It will be noted that these are in general smaller than those for the United Kingdom data. This is to be expected, for the winter months in California are comparatively uniform in their rainfall regimes.

At Saint John, New Brunswick, the variation through the year in the probability of the weather following a wet day is small. Under these circumstances, the wet periods during the year were combined into one frequency distribution. Table 3 gives the combined distribution, and the values obtained from the trend line

$$\log y = 4{\cdot}0637 - 0{\cdot}3171\,n$$

A comparison between the values of columns 3 and 4 in Table 3 shows that the departures are small. When, then, the probabilities are equal, it is possible to combine the series and derive a series which gives approximately a straight line.

TABLE 3. Wet periods at Saint John, total for the year

| Days ($n$) | Wet periods lasting $n$ days or longer ($y$) | $\log y$ | Computed $\log y$ |
|---|---|---|---|
| 1 | 5295 | 3·724 | 3·747 |
| 2 | 2939 | 3·467 | 3·430 |
| 3 | 1343 | 3·128 | 3·112 |
| 4 | 620 | 2·792 | 2·795 |
| 5 | 289 | 2·461 | 2·478 |
| 6 | 141 | 2·149 | 2·161 |
| 7 | 66 | 1·820 | 1·844 |
| 8 | 36 | 1·556 | 1·527 |
| 9 | 16 | 1·208 | 1·210 |
| 10 | 8 | 0·903 | 0·893 |

## 6. Computation of the probabilities

The method described above involves a great deal of work. It is necessary to study the daily precipitation record for a number of years. Probably 30 years is a minimum if a reliable value is to be obtained. From these daily precipitation records, the frequency distributions of the lengths of dry and wet periods must be obtained. From the frequency distributions, one computes the trend lines, and from the equations determines the probabilities. There is an easier way.

From the frequency distributions of the cumulative frequencies of dry periods the total number of dry days during the period under study is given by $\Sigma y$, including of course the values which were not used in computing the trend line. Of these dry days, there were a number which were followed by wet days. This number equals the number of wet periods. Then the probability of a wet day following a dry day is given by the ratio, number of wet periods/number of dry days. The probability of a dry day following a dry day is obtained by subtracting the ratio from unity. In a similar manner the probabilities for the weather following a wet day may be determined.

The two methods are not identical although they are based on the same data, and the results are not the same. Table 4 gives probabilities (in per cent) as computed by the two methods for the five Canadian cities. In most cases, the two values are not greatly different. The differences are greater for those following dry days than following wet days. These reflect the conclusion that the trend lines fit the data for wet periods better than for dry periods. Of the 120 pairs, seven have differences 7 or more. The difference of 12 for December dry days for Victoria is the greatest.

TABLE 4. PROBABILITY (PER CENT) OF A WET DAY FOLLOWING A DRY DAY

(a) based on slope of trend line, (b) based on number of wet periods and dry days

| | | J | F | M | A | M | J | J | A | S | O | N | D |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Saint John | (a) | 40 | 44 | 39 | 33 | 34 | 34 | 38 | 30 | 34 | 35 | 38 | 45 |
| | (b) | 43 | 40 | 39 | 34 | 33 | 36 | 35 | 33 | 33 | 36 | 38 | 41 |
| Montreal | (a) | 45 | 42 | 35 | 29 | 30 | 34 | 37 | 31 | 30 | 30 | 38 | 43 |
| | (b) | 50 | 43 | 37 | 32 | 33 | 35 | 35 | 33 | 33 | 34 | 43 | 46 |
| Winnipeg | (a) | 20 | 19 | 18 | 20 | 23 | 33 | 31 | 26 | 18 | 19 | 22 | 21 |
| | (b) | 24 | 23 | 20 | 21 | 26 | 35 | 31 | 29 | 24 | 20 | 23 | 23 |
| Dawson | (a) | 16 | 11 | 11 | 14 | 20 | 20 | 24 | 25 | 21 | 19 | 20 | 17 |
| | (b) | 23 | 17 | 14 | 14 | 22 | 27 | 28 | 28 | 25 | 23 | 23 | 20 |
| Victoria | (a) | 31 | 26 | 29 | 21 | 14 | 11 | 7 | 14 | 18 | 28 | 39 | 29 |
| | (b) | 37 | 34 | 35 | 25 | 19 | 16 | 8 | 15 | 20 | 34 | 44 | 41 |

PROBABILITY (PER CENT) OF A DRY DAY FOLLOWING A WET DAY

(a) based on slope of trend line, (b) based on number of dry periods and wet days

| | | J | F | M | A | M | J | J | A | S | O | N | D |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Saint John | (a) | 57 | 54 | 52 | 53 | 49 | 49 | 52 | 51 | 51 | 50 | 48 | 57 |
| | (b) | 51 | 51 | 48 | 49 | 49 | 47 | 51 | 47 | 51 | 52 | 48 | 50 |
| Montreal | (a) | 45 | 42 | 45 | 52 | 52 | 53 | 55 | 55 | 44 | 58 | 47 | 46 |
| | (b) | 44 | 44 | 44 | 46 | 47 | 48 | 53 | 55 | 50 | 50 | 43 | 43 |
| Winnipeg | (a) | 64 | 56 | 62 | 58 | 59 | 56 | 61 | 63 | 56 | 57 | 52 | 57 |
| | (b) | 61 | 59 | 62 | 56 | 56 | 54 | 62 | 59 | 57 | 58 | 56 | 58 |
| Dawson | (a) | 56 | 65 | 61 | 63 | 56 | 47 | 47 | 49 | 57 | 54 | 40 | 56 |
| | (b) | 57 | 62 | 66 | 69 | 54 | 49 | 48 | 50 | 55 | 56 | 50 | 56 |
| Victoria | (a) | 26 | 33 | 36 | 53 | 51 | 55 | 57 | 57 | 48 | 28 | 22 | 24 |
| | (b) | 28 | 32 | 34 | 51 | 51 | 56 | 58 | 49 | 44 | 30 | 24 | 23 |

The comparison of Table 4 suggests that the desired probabilities might be obtained more easily than by the method of this study, by using the number of dry and wet days and periods. A shorter period than 30 years might give an approximately correct value of the desired probability. This method gives a good approximation only because the probabilities do not change greatly with the length of period.

## ACKNOWLEDGMENTS

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## REFERENCES

Jorgensen, D. L. 1949 *Mon. Weath. Rev., U.S.W.B.*, **77**, p. 303.
Newnham, E. V. 1916 *Quart. J. R. Met. Soc.*, **42**, p. 153.

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# Upper winds of Aden

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## 1. Introduction

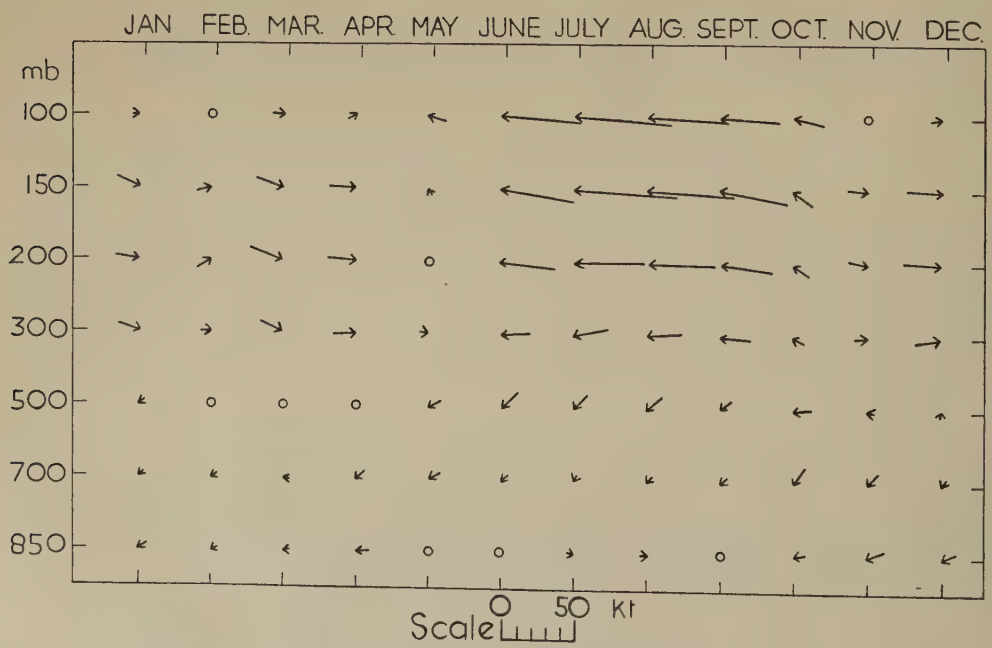
Summaries of the monthly frequencies of wind at Aden (12° 49′N, 45° 02′E) for the period Aug. 1948 to Dec. 1950 have been compiled in the Upper Air Climatology Branch of the Meteorological Office, with a view to publication in the series of volumes of *Upper air data* now in course of preparation. The results show many points of interest, and in view of the scarcity of information about wind at high levels in the tropics a summary of the main features in advance of the publication of the complete tables may be of value.

## 2. The observations

Radar-wind observations began at Aden in Aug. 1948. For the first nine months (Aug. 1948 to Apr. 1949) ascents were made only once a day at 15 GMT (18 Zone Time), but from May onwards a second observation was made at 03 GMT (06 Zone Time).

TABLE 1. Number of observations in each month

| mb | J | F | M | A | M | J | J | A | S | O | N | D | Year |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 100 | 63 | 57 | 66 | 78 | 87 | 70 | 72 | 84 | 114 | 130 | 122 | 122 | 1065 |
| 200 | 84 | 74 | 85 | 86 | 103 | 95 | 96 | 124 | 134 | 145 | 140 | 147 | 1313 |
| 850 | 87 | 80 | 88 | 89 | 106 | 102 | 103 | 128 | 143 | 150 | 144 | 151 | 1371 |




o = vector velocity 2 kt or less

Figure 1. Vector resultant winds at Aden, 1948-50.

From these observations frequency tables were compiled for fourteen levels, viz., surface, 900, 850, 800, 700, 600, 500, 400, 300, 200, 150, 100, 60, 40 mb. Only 208 of the 1371 ascents reached 60 mb and only 5 reached 40 mb.

In working up the frequency tables all winds of less than 10 kt were counted as calms. This was done mainly for ease in mechanical computation but partly also because the direction of the very light winds is often doubtful, especially at high levels.

From the frequencies (treating winds of less than 10 kt as calms) mean vector winds, standard vector deviations and mean scalar speeds were computed. The results are shown in Table 2 and Fig. 1.

TABLE 2. DIRECTION AND SPEED OF UPPER WINDS, ADEN, 12° 49′N, 45° 02′E, 1948-50

| Pressure level (mb) | J | F | M | A | M | J | J | A | S | O | N | D |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | Mean vector wind (degrees, kt) | | | | | | |
| 100 | 270 | 227 | 277 | 247 | 116 | 94 | 94 | 93 | 93 | 101 | calm | 264 |
| | 5 | 2 | 7 | 6 | 12 | 55 | 66 | 54 | 39 | 18 | | 7 |
| 150 | 293 | 257 | 291 | 271 | 147 | 99 | 93 | 93 | 100 | 124 | 275 | 273 |
| | 15 | 10 | 19 | 18 | 3 | 49 | 71 | 60 | 46 | 15 | 14 | 23 |
| 200 | 279 | 241 | 291 | 273 | 290 | 96 | 90 | 91 | 98 | 125 | 281 | 273 |
| | 17 | 11 | 25 | 19 | 2 | 36 | 48 | 45 | 36 | 13 | 14 | 26 |
| 300 | 289 | 270 | 296 | 268 | 279 | 88 | 80 | 87 | 94 | 115 | 269 | 262 |
| | 15 | 7 | 18 | 16 | 6 | 21 | 23 | 24 | 20 | 8 | 8 | 17 |
| 500 | 59 | 77 | 166 | 62 | 66 | 48 | 45 | 51 | 53 | 85 | 99 | 192 |
| | 3 | 1 | 1 | 1 | 9 | 16 | 13 | 13 | 10 | 12 | 3 | 3 |
| 700 | 34 | 56 | 101 | 56 | 64 | 53 | 29 | 49 | 52 | 35 | 43 | 39 |
| | 3 | 4 | 3 | 8 | 9 | 7 | 3 | 4 | 7 | 15 | 9 | 5 |
| 850 | 63 | 64 | 86 | 87 | 99 | 236 | 278 | 267 | 258 | 80 | 68 | 63 |
| | 7 | 6 | 5 | 10 | 1 | 2 | 5 | 5 | 1 | 7 | 13 | 11 |
| | | | | | | Vector standard deviation (kt) | | | | | | |
| 100 | 18 | 16 | 17 | 15 | 19 | 22 | 20 | 18 | 18 | 16 | 15 | 21 |
| 150 | 22 | 23 | 21 | 21 | 26 | 21 | 14 | 22 | 21 | 19 | 20 | 27 |
| 200 | 25 | 25 | 29 | 23 | 27 | 19 | 15 | 16 | 18 | 17 | 23 | 31 |
| 300 | 19 | 20 | 22 | 20 | 21 | 12 | 12 | 8 | 11 | 13 | 19 | 23 |
| 500 | 13 | 12 | 10 | 13 | 14 | 13 | 11 | 10 | 11 | 16 | 13 | 15 |
| 700 | 6 | 11 | 10 | 12 | 13 | 11 | 9 | 10 | 11 | 10 | 13 | 11 |
| 850 | 9 | 10 | 11 | 12 | 8 | 9 | 10 | 10 | 8 | 10 | 7 | 9 |
| | | | | | | Mean scalar speed (kt) | | | | | | |
| 100 | 16 | 14 | 15 | 14 | 18 | 55 | 66 | 54 | 40 | 21 | 13 | 18 |
| 150 | 23 | 21 | 24 | 23 | 22 | 50 | 71 | 61 | 47 | 21 | 20 | 29 |
| 200 | 26 | 23 | 31 | 26 | 23 | 37 | 50 | 46 | 37 | 19 | 22 | 36 |
| 300 | 22 | 18 | 24 | 22 | 19 | 22 | 25 | 24 | 22 | 14 | 18 | 25 |
| 500 | 12 | 10 | 8 | 12 | 14 | 19 | 16 | 16 | 13 | 15 | 12 | 13 |
| 700 | 10 | 10 | 9 | 13 | 14 | 11 | 9 | 9 | 11 | 17 | 14 | 11 |
| 850 | 10 | 10 | 9 | 13 | 7 | 8 | 10 | 10 | 7 | 10 | 15 | 14 |

There are many interesting features, but outstanding is the strong easterly wind above 300 mb in the summer months which, as first noted by Frost (1952), reaches its maximum between 150 mb and 100 mb in July when it averages over 70 kt. Easterly winds of similar strength have been reported also over India in summer (Krishna Rao 1952), average speeds of over 80 kt being observed between 7°N and 15°N at about 16 km to 18 km (80-100 mb). It is of interest also to note that in his report of the expedition organized by the Finnish Academy of Sciences in 1939, Vuorela (1950) gives a cross section over the Atlantic for August-September showing easterly winds of over 80 kt at about 13·5 km (150 mb) in 8°N; these he associates with the regeneration of the sub-tropical high and consequent strengthening of the zonal index.

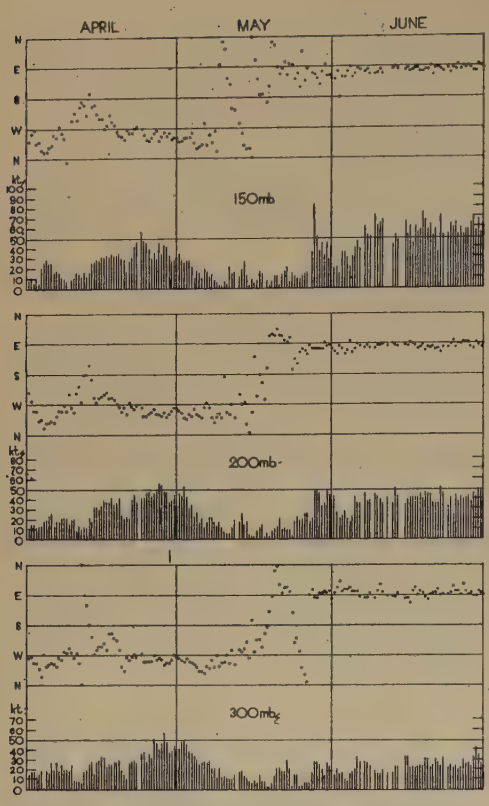



Figure 2. Transition from westerly to easterly winds at Aden in 1950 at the levels of 300, 200 and 150 mb.

### 3. Transition to easterlies in May

At Aden the onset of the upper easterlies takes place in May and is fairly abrupt. Fig. 2 shows the period of transition in 1950 at the levels of 300, 200 and 150 mb. Values of direction and speed are plotted at 03 and 15 GMT (06 and 18 Zone Time) from 1 April to 30 June. The transition to persistent easterlies in the latter part of May is clearly shown. The time of change varies slightly from year to year, in 1949 and 1950 easterlies became established at 200 mb about 18 May but in 1951 not until 25 May.

Observations at Bahrein (26° 16′N, 50° 37′E) show that a somewhat similar transition occurs there but at a later date : in 1950 winds at 200 mb were westerly until about 9 June, light and variable from 9 to 21 June and easterly thereafter.

### 4. Persistent easterlies in July

The easterlies at Aden reach their greatest development in July (Fig. 3) when their average speed at 150 mb is over 70 kt. In July 1950 the highest speed exceeded 70 kt in every ascent and on six days exceeded 100 kt; the average was 92 kt. The level at which the highest speed was reached is shown in Table 3 which gives the frequency with which the maximum speed occurred between different pressure levels :

TABLE 3. Frequencies of maximum speeds at different pressure levels

| mb | 80-99 | 100-119 | 120-139 | 140-159 | 160-169 |
|---|---|---|---|---|---|
| Frequency | 7 | 18 | 21 | 4 | 2 |

(Six ascents are omitted because the wind was still increasing when observations ceased; in all these the ascent reached 200 mb and in 4 it reached 150 mb).

### 5. Transition to westerlies in October

At Aden easterlies persist through the greater part of October, becoming lighter and more variable in direction (Fig. 4). The approximate dates of their cessation in the three years 1949-51 were : 21 October 1949, 23 October 1950, 12 October 1951. In 1949 and 1951 they were followed by a period of two or three weeks of light variable winds before the westerlies became established.

At Bahrein in 1950 the change from easterlies to westerlies at 200 mb took place in the last week in August, but light easterlies were recorded again for a few days in the early part of September.

### 6. Relation to pressure gradient

The onset of the easterly wind at Aden appears to be related to the heating up of the continent further north and the associated rise in the level of the isobaric surfaces (Fig. 5).

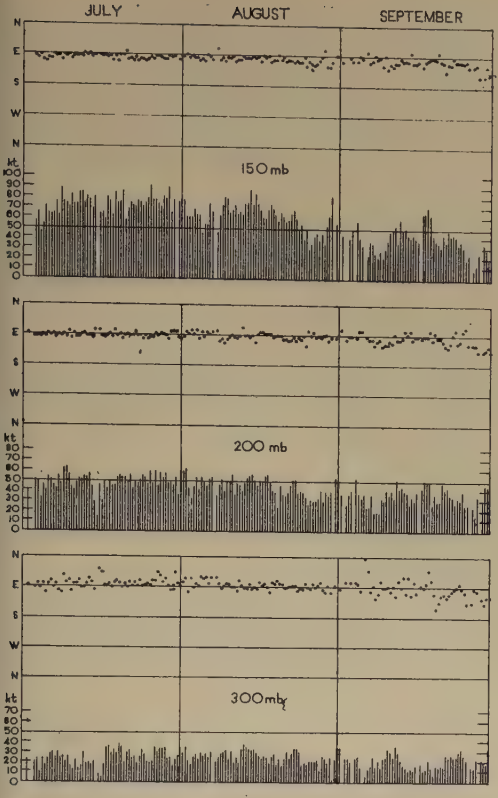



Figure 3. Persistent easterlies at Aden in 1950 (July-September) at the levels of 300, 200 and 150 mb.

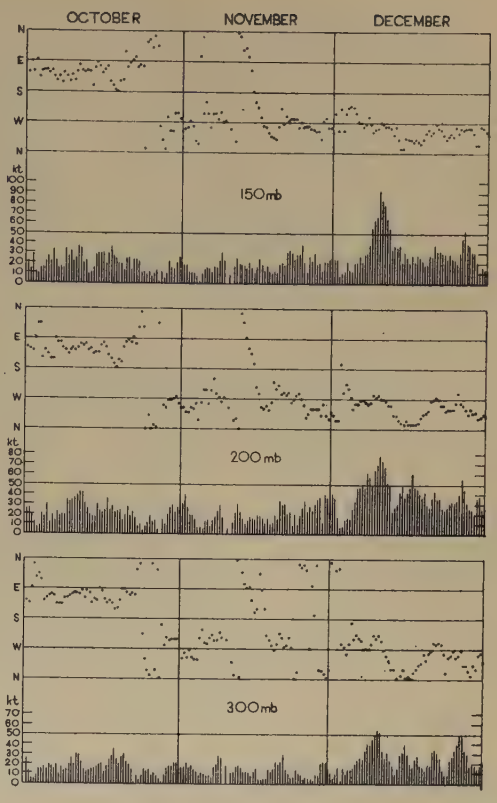



Figure 4. Transition from easterly to westerly winds at Aden in 1950 at the levels of 300, 200 and 150 mb.

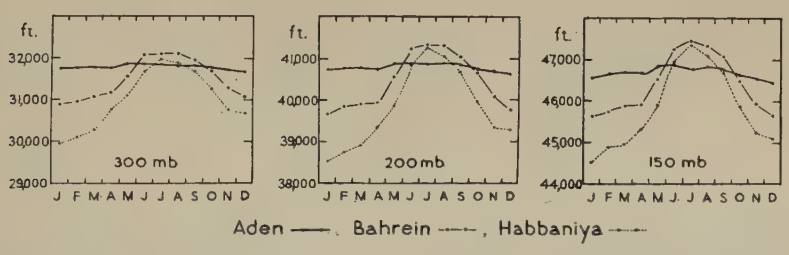



Figure 5. Mean height in 1950 of isobaric surfaces at Aden, Bahrein and Habbaniya.

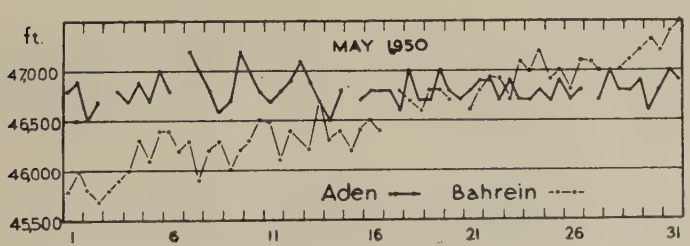



Figure 6. Height of the 150 mb isobaric surface at Aden and Bahrein in May 1950.

It is noteworthy that the level at Aden shows little change through the year whereas at Bahrein and still more at Habbaniya there is a marked annual variation. The result is that whereas in the colder part of the year, October to May, the height is greater at Aden that at Bahrein, i.e. the gradient is for westerly winds, in the remaining months June to September the reverse is true and the gradient is for easterly winds. It appears that during the hot season the isobaric surfaces are at their highest level somewhere between Bahrein and Habbaniya; easterly winds would therefore be expected at Bahrein on the southern side and westerly winds at Habbaniya on the northern side, and this is in accord with the observations.

In Fig. 6 twice-daily values of the height of the 150-mb surface are plotted for May 1950 at Aden and Bahrein; the reversal of the gradient in the last fortnight of the month is clearly shown.

### Acknowledgments

The computations on which this note is based were carried out by the staff of the Upper Air Climatology Branch of the Meteorological Office. For permission to publish the note thanks are due to the Director of the Meteorological Office.

### References

Frost, R. 1952 *Prof. Notes, Met. Off.*, London, No. 107.
Krishna Rao, P. R. 1952 *Curr. Sci.*, Bangalore, **21**, Pp. 63-4.
Vuorela, L. A. 1948 *J. Met.*, **5**, p. 115.
1950 The Aerological Atlantic Expedition of the Finnish Academy of Sciences, Part II., *Mitt. Met. Inst. Univ. Helsingfors*, No. 67.

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551.526

# On a relation between the annual heat budgets of seas and lakes and other terms of the energy equation

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## 1. Definitions

The annual heat budget $Q_H$ of a lake is defined (e.g., Welch 1935) as the amount of heat in g cal needed to raise the mean temperature of a water column of 1 cm$^2$ cross-section and of depth $\overline{D}$ cm ($\overline{D}$ is the mean depth of the lake) from its minimum temperature $\overline{T}_n$ late in winter to its maximum temperature $\overline{T}_x$ late in summer, or

$$Q_H = \overline{D}\,(\overline{T}_x - \overline{T}_n)$$

The definition can be extended to apply to sea and ocean areas as well.

To find the value of the above heat budget, the annual variation of temperature with depth must be known, or rather, a knowledge of the values of $\overline{T}_x$ and $\overline{T}_n$ is required. For average conditions, however, the energy equation enables us, as is shown below, to compute the heat budget if the respective dates of occurrence but not the actual values of $\overline{T}_x$ and $\overline{T}_n$ are available.

Let $E$ = amount of evaporation g/cm$^2$ or in cm

$Q$ = energy in g cal/cm$^2$ available for transfer by conduction and convection to the atmosphere and to sub-surface layers of the sea

$Q_c$ = energy conducted from the sea to the atmosphere (transfer of sensible heat)

$Q_e$ = energy transferred from the sea to the atmosphere by evaporation (transfer of latent heat)

$L$ = latent heat of vaporization in g cal/g at average sea-surface temperature of the period concerned

$R$ = Bowen's ratio $= \dfrac{Q_c}{Q_e}$ (e.g., Sverdrup *et al.* 1946)

Without primes, the above symbols will represent values of the variables for an annual cycle. The prime ′ will refer to the period between the dates of occurrence of the mean sea-column temperature minimum $\overline{T}_n$ and that of the maximum $\overline{T}_x$; we shall refer to that period as Period 1. The double prime ″ will refer to the remaining period of an annual cycle to be called Period 2.

## 2. An expression for the annual heat budget

For an average annual cycle, the equation

$$E = E' + E''$$

can be rewritten in terms of the energy equation in the form

$$\frac{Q}{L(1+R)} = \frac{Q' - Q_H}{L'(1+R')} + \frac{Q'' + Q_H}{L''(1+R'')} \quad . \quad . \quad . \quad (1)$$

where

$$Q = Q' + Q''. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Making the reasonable approximation $L = L' = L''$, Eq. (1) becomes

$$\frac{Q}{1+R} = \frac{Q' - Q_H}{1+R'} + \frac{Q'' + Q_H}{1+R''} \quad . \quad . \quad . \quad . \quad (3)$$

If $R = R' = R''$, Eq. (3) reduces to Eq. (2) and $Q_H$ cannot be determined. The condition $R = R' = R''$ is most nearly fulfilled in the neighbourhood of the thermal equator where sea-surface and air temperatures, humidity of the air and atmospheric pressure, all of which are involved in calculations of Bowen's ratio, have little seasonal variation. In this zone, moreover, the annual variation of solar radiation is a double wave and, owing to this, the annual variation of heat content of the water passes, in all probability, through two cycles in the course of one year. Therefore, before applying Eq. (3) to data of the equatorial zone, the periods adopted in this note would have to be redefined.

Inserting (2) into (3), the following symmetrical expression results for $Q_H$:

$$Q_H = \frac{(1+R'')(R-R')Q' - (1+R')(R''-R)Q''}{(1+R)(R''-R')} \quad . \quad . \quad (4)$$

## 3. Discussion

Eq. (4) is particularly useful in connection with seas (or lakes) for which terms of the energy balance representing advection of heat by the inflowing or outflowing water masses are negligible at any time of the annual cycle. For such water bodies, $Q$, $Q'$ and $Q''$ reduce to the net radiation balances at the sea surface for the periods concerned. In this case, the approximate values of $Q_H$ may be computed with the aid of Eq. (4) by recourse solely to surface data, including temperatures and salinities of the sea surface. No knowledge of the annual variation of temperature below sea surface is required, but an assumption about the dates of $\overline{T}_n$ and $\overline{T}_x$ is needed.

In practical applications a shortcoming of Eq. (4) is the fact that $Q_H$ is obtained as the difference of two terms the error in either of which may seriously influence the value of $Q_H$ thus computed. For a closer consideration of this problem, we can rewrite (3) with the aid of (2) in the following form:

$$Q_H = Q' - \left(\frac{R''-R}{R''-R'}\right)\left(\frac{1+R'}{1+R}\right)Q \quad . \quad . \quad . \quad (5)$$

For low latitudes where the annual variation of the meteorological elements is relatively small, there will be little difference between $R$ and $R'$. Hence, in the second term of the

right-hand side of Eq. (5), the factor $(1 + R')/(1 + R)$ will be practically equal to unity, whereas the factor $(R'' - R)/(R'' - R')$ will be about $\frac{1}{2}$. Thus, Eq. (5) becomes

$$Q_H \simeq Q' - \tfrac{1}{2}Q \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

$Q \simeq 2Q'$ so that $Q_H$ is obtained as a small difference of two large terms of comparable magnitude, the error in either of which could easily be of the order of the difference being sought.

The position with respect to middle and moderately high latitudes is more favourable. Here $R'' > R > 0$, $R' < 0$ and $|R'| \simeq R$, the last being a somewhat rough representation of actual conditions. The factor $(1 + R')/(1 + R) \simeq \frac{1}{2}$ and, as in the former case, $(R'' - R)/(R'' - R') \simeq \frac{1}{2}$. With these approximations, Eq. (5) reduces to

$$Q_H \simeq Q' - \tfrac{1}{4}Q. \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Since in middle latitudes in winter, the sea surface loses more heat than it gains by radiation, the net radiation balance at its surface for Period 2 *as a whole* is small so that $Q$ and $Q'$ are of comparable magnitude. Hence, the second term on the right-hand side of Eq. (7) is of the order of one-fourth of the first term by which $Q_H$ is primarily determined. It would thus appear that the percentage error of $Q_H$ is about the same as the percentage error of determination of $Q'$, and, for the middle latitudes, is likely to be less than was the case for lower latitudes.

Since, for the poleward side of the middle latitudes, $Q \simeq Q'$, we could simplify Eq. (7) to the form

$$Q_H \simeq \tfrac{3}{4}Q' \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

(8) is of course, a much cruder approximation to (5) than is (7). We may test its usefulness on the data for an area of the eastern North Atlantic about 47°N, 12°W whose energy balance was studied by Sverdrup (1940). We shall assume that the minimum temperature of the water column is reached in mid-March and that the maximum temperature occurs at the end of August. The actual dates are slightly earlier especially in the case of the maximum temperature.

Sverdrup considers the advection of heat by currents to be negligible for that ocean area. Hence, $Q$ reduces to the net radiation balance at the sea surface. From Sverdrup's graph (1940, Fig. 31a, p. 95) we find that $Q'$ (radiation balance between mid-March and the end of August) is about 45,000 g cal/cm$^2$ whereas $Q$ is about 50,000 g cal/cm$^2$. Using these data to compute $Q_H$ (in g cal/cm$^2$), (7) gives 32,500 while (8) yields 33,750. The observed $Q_H$ is approximately 32,000 g cal/cm$^2$.

## References


Sverdrup, H. U. 1940 'On the annual and diurnal variation of evaporation from the oceans,' *J. Mar. Res.*, **3**, p. 93.

Sverdrup, H. U., Johnson, M. W. and Fleming, R. H. 1946 *The oceans*, New York (Prentice-Hall).

Welch, P. S. 1935 *Limnology*, New York (McGraw-Hill).


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# How much does a gale mix the surface layers of the ocean?


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(Manuscript received 28 May 1953)

It is commonly accepted that wind is a major cause of the mixing in the upper layers of the ocean. A qualitative statement of this idea is given by Sverdrup (1942), with schematic diagrams showing the deepening and the weakening of the thermocline after a period of wind. Mixing due to winds of Beaufort Force less than 6, and confined to the upper 50 ft, has been apparently observed in the Gulf of Mexico (Stommel and Woodcock 1951). An example of mixing due to winds of hurricane force in coastal waters has been reported by Iselin (1939). In order to discover whether in fact a gale produces detectable changes in the depth and intensity of the thermocline

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in the deep ocean, we have examined the bathythermograph records of 502 weather-ship cruises in the North Atlantic. In selecting case histories from the records the following criteria were applied :

1. The weather ship had to be in the same position throughout the preceding calm weather and the gale.
2. Weather stations A (62°N, 33°W), C (52°45′N, 35°30′W) and E (now 34°N, 52°W; before 3 Sept. 1949, 35°N, 48°W) were the only ones considered because they are in areas where the horizontal gradients of temperature and density are most likely to be small.
3. There is no thermal structure in the upper layers during the winter, therefore only the period from May to October was examined.
4. The winds for at least two days were to be below Beaufort Force 4 followed by at least one day of Force 8 or more.

It is not surprising that from such a large amount of bathythermograph data only 15 series of records were found satisfying these criteria, for in stormy weather the regular observations are often suspended.

TABLE 1. TEMPERATURES (°F) IN THE UPPER LAYERS OF THE DEEP OCEAN BEFORE AND AFTER A GALE

| Cruise No. | | 113A | | 114A | | 115 | | 118 | | 125 | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Station | | C | | E | | C | | A | | C | |
| Greatest wind force | | 8 | | 8 | | 8 | | 8 | | 8 | |
| Year | | 1946 | | 1946 | | 1946 | | 1946 | | 1946 | |
| Month | | May | | May | June | July | | August | | October | |
| Day | | 3 | 5 | 29 | 1 | 19 | 21 | 19 | 20 | 16 | 19 |
| Greenwich Time | | 1200 | 1200 | 1200 | 1200 | 1100 | 0000 | 1200 | 1530 | 1815 | 1200 |
| Depth (ft) | 0 | 46·3 | 46·0 | 71·9 | 70·7 | 53·2 | 52·7 | 49·5 | 49·7 | 50·5 | 51·7 |
| | 25 | 45·7 | 45·7 | **71·8** | 70·7 | 53·2 | 52·7 | 49·5 | 49·7 | 50·5 | 51·7 |
| | 50 | 45·5 | 45·7 | 69·0 | **70·7** | **53·2** | 52·7 | 49·5 | 49·7 | 50·5 | 51·7 |
| | 75 | **45·5** | 45·7 | 68·0 | 69·0 | 52·3 | 52·7 | **49·5** | 49·7 | 50·3 | **51·7** |
| | 100 | 45·0 | **45·7** | 67·5 | 67·2 | 51·7 | **52·7** | 49·0 | **49·7** | 50·3 | 50·7 |
| | 150 | 44·5 | 44·7 | 66·2 | 66·8 | 51·2 | 51·3 | 43·8 | 47·0 | **50·3** | 50·6 |
| | 200 | 44·2 | 44·3 | 65·0 | 65·0 | 48·9 | — | 43·0 | 44·0 | 49·3 | 50·5 |
| | 300 | 42·5 | 44·3 | 63·9 | 63·5 | 47·2 | — | 42·8 | 44·0 | 45·3 | 44·8 |

| Cruise No. | | 156 | | 180 | | 182 | | 245 | | 258 | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Station | | C | | C | | A | | C | | C | |
| Greatest wind force | | 8 | | 9 | | 8 | | 8 | | 9 | |
| Year | | 1947 | | 1948 | | 1948 | | 1949 | | 1949 | |
| Month | | August | | June | | June | | June | | August | |
| Day | | 15 | 18 | 2 | 4 | 17 | 21 | 20 | 22 | 2 | 4 |
| Greenwich Time | | 1200 | 1200 | 1200 | 1700 | 1200 | 1600 | 1500 | 1200 | 1400 | 1400 |
| Depth (ft) | 0 | 50·0 | 50·0 | 46·8 | 46·0 | 46·8 | 46·0 | 47·8 | 47·2 | 51·1 | 51·1 |
| | 25 | 50·0 | 50·0 | 46·8 | 46·0 | 46·2 | 46·0 | 47·7 | 47·2 | 51·1 | 51·1 |
| | 50 | 50·0 | 50·0 | **46·5** | 46·0 | 46·1 | 46·0 | 47·7 | 47·2 | 50·8 | 51·1 |
| | 75 | 50·0 | 50·0 | 45·2 | **45·8** | 46·0 | 46·0 | **47·2** | 47·1 | 50·8 | 51·1 |
| | 100 | **50·0** | **50·0** | 45·0 | 45·7 | 46·0 | **46·0** | 46·0 | **47·1** | **50·6** | **51·1** |
| | 150 | 49·5 | 47·5 | 44·2 | 44·2 | **46·0** | 44·6 | 44·5 | 45·9 | 46·8 | 48·0 |
| | 200 | 45·7 | 44·0 | 44·0 | 43·9 | 44·8 | — | 43·8 | 44·0 | 45·2 | 45·7 |
| | 300 | 44·3 | 42·7 | — | — | 44·2 | — | 42·1 | 42·5 | 43·0 | 43·8 |

| Cruise No. | | 262 | | 327 | | 362 | | 443 | | 461 | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Station | | C | | C | | C | | A | | C | |
| Greatest wind force | | 8 | | 8 | | 12 | | 8 | | 9 | |
| Year | | 1949 | | 1950 | | 1950 | | 1951 | | 1951 | |
| Month | | August | | May | | September | | July | | September | |
| Day | | 9 | 12 | 1 | 3 | 22 | 24 | 17 | 19 | 10 | 14 |
| Greenwich Time | | 1130 | 1800 | 1000 | 1400 | 0900 | 0900 | 1800 | 1000 | 1000 | 1000 |
| Depth (ft) | 0 | 52·8 | 52·0 | 44·1 | 43·5 | 51·8 | 51·6 | 48·5 | 47·8 | 52·1 | 50·0 |
| | 25 | 52·8 | 51·9 | 44·1 | 43·4 | 51·8 | 51·6 | 48·5 | 47·8 | 52·1 | 50·0 |
| | 50 | 52·5 | 51·9 | 44·1 | 43·4 | 51·8 | 51·6 | 48·5 | 47·8 | 52·1 | 50·0 |
| | 75 | 52·0 | 51·9 | 44·0 | 43·4 | 51·8 | 51·5 | **48·3** | 47·8 | 52·1 | 50·0 |
| | 100 | **52·0** | **51·6** | **44·0** | 43·4 | 51·8 | 51·4 | 47·0 | **47·8** | 52·1 | 50·0 |
| | 150 | 50·0 | 47·0 | 42·8 | 43·3 | 51·8 | 51·4 | 43·5 | 44·3 | **52·1** | **50·0** |
| | 200 | 46·0 | 45·7 | 42·8 | **43·2** | **51·8** | **51·2** | 43·2 | 43·0 | 46·2 | 47·8 |
| | 300 | 43·0 | 43·2 | 42·1 | 42·0 | 45·2 | 46·2 | 43·0 | 41·0 | 43·0 | 43·0 |

Data from these 15 series are listed in Table 1 which gives the temperatures, at a number of selected depths, from the slide immediately before the gale and from another slide after the gale has been blowing for some time, the thermocline being indicated by bold type for each sounding. Even in the absence of gales there are normally fluctuations in the observed depth of the thermocline and, indeed, in the temperature, of the deep water beneath, often amounting to 1°F. These are apparently due in part to the crudeness of the instrument and in part to heterogeneities and internal wave motions in the water mass itself. Moreover, the weather ships used 450 ft depth bathythermographs so that it is difficult to read with any precision the thermal structure of the upper 25 ft which is often obscured by the vibration of the pen.

Nevertheless, it seems clear that we can draw the following conclusions from these data. A single gale may deepen the mixed surface layer by about 20 ft to 30 ft on the average. This deepening may occasionally be obscured by changes in the thermal structure due to causes about which we can only speculate. A rough computation of the increase of potential energy due to the mixing indicates that the change in all fifteen cases in the deep ocean was markedly less than that in the coastal water reported by Iselin (1939). The significance of this difference is doubtful, however, because 26 days elapsed between the two observations of the coastal water.

## References


Iselin, C. O'D. 1939 'Some physical factors which may influence the productivity of New England's coastal waters,' *J. Mar. Res.*, **2**, p. 74.

Stommel, H. and Woodcock, A. H. 1951 'Diurnal heating of the surface of the Gulf of Mexico in the spring of 1942,' *Trans. Amer. Geophys. Un.*, **32**, p. 565.

Sverdrup, H. U., Johnson, M. W. and Fleming, R. H. 1942 '*The oceans*,' New York, (Prentice-Hall).


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551.577.36 : 551.501.45

# The duration of wet and dry spells at Moncton, New Brunswick*

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## Summary

It is shown that the frequency distributions of dry and wet spells at Moncton, N.B., conform very closely to geometric and logarithmic series, respectively.


## 1. Introduction

Several authors, notably Gold (1929) and Williams (1952), have investigated the frequency distribution of wet and dry sequences (spells) of various lengths. Grouping both the wet and dry spells, Gold has shown that the distribution does not follow a geometric progression of the form $y = \alpha x^n$ where $\alpha$ is a constant, $x$ is a constant which is always less than unity, $n$ is the number of days in the spell and $y$ is the number of sequences containing $n$ days. Williams, however, has shown that both wet and dry spells, considered separately, have distributions which conform very closely to a ' logarithmic ' series of the form $y = \alpha x^n/n$ where $\alpha$, $x$ and $n$ have the same meanings as above.

A study of the frequency distribution for Moncton, N.B., indicates that dry-spell data fit very closely the geometric progression $y = 1332\ (0{\cdot}7272)^n$ and wet-spell data fit the logarithmic curve $y = 4391\ (0{\cdot}551)^n/n$.

* Published by permission of the Controller, Meteorological Division, Department of Transport, Canada

† Formerly of the Meteorological Division, Dept. of Transport, Canada.

TABLE 1. FREQUENCY DISTRIBUTION OF SPELLS OF WET AND DRY DAYS AT MONCTON, NEW BRUNSWICK, DURING THE YEARS 1900-1949 INCLUSIVE, TOGETHER WITH CORRESPONDING VALUES CALCULATED FROM GEOMETRIC (FOR DRY DAYS) AND LOGARITHMIC (FOR WET DAYS) SERIES

DRY

| Length of spell in days | Winter Dec.-Feb. | Spring Mar.-May | Summer Jun.-Sept. | Autumn Oct.-Nov. | 50 years Observed | 50 years Calculated |
|---|---|---|---|---|---|---|
| 1 | 189 | 231 | 317 | 146 | 883 | 968 |
| 2 | 178 | 186 | 247 | 132 | 743 | 705 |
| 3 | 142 | 147 | 175 | 86 | 550 | 513 |
| 4 | 102 | 115 | 141 | 77 | 435 | 374 |
| 5 | 80 | 59 | 93 | 48 | 280 | 272 |
| 6 | 45 | 41 | 63 | 29 | 178 | 198 |
| 7 | 31 | 33 | 48 | 18 | 130 | 144 |
| 8 | 24 | 21 | 28 | 15 | 88 | 105 |
| 9 | 14 | 18 | 33 | 15 | 80 | 76·4 |
| 10 | 15 | 14 | 21 | 4 | 54 | 55·1 |
| 11 | 14 | 7 | 13 | 4 | 38 | 40·4 |
| 12 | 6 | 7 | 7 | 5 | 25 | 29·4 |
| 13 | 5 | 5 | 4 | 3 | 17 | 21·4 |
| 14 | 2 | 3 | 4 | 4 | 13 | 15·6 |
| 15 | 3 | 3 | 3 | — | 9 | 11·4 |
| 16 | 2 | 4 | — | 1 | 7 | 8·3 |
| 17 | 2 | 3 | 2 | 2 | 9 | 6·1 |
| 18 | — | 1 | 1 | 1 | 3 | 4·4 |
| 19 | 1 | — | — | 1 | 2 | 3·2 |
| 20 | — | 1 | 1 | — | 2 | 2·3 |
| 21 | — | — | — | — | — | 1·7 |
| 22 | 1 | — | 1 | — | 2 | 1·2 |
| 23 | — | — | 1 | — | 1 | ·9 |
| 24 | 1 | — | — | — | 1 | ·6 |
| 25 | — | — | — | — | — | ·5 |
| 26 | — | — | — | — | — | ·3 |
| 27 | — | — | — | — | — | ·3 |
| 28 | — | — | — | — | — | ·2 |
| 29 | 1 | — | — | — | 1 | ·1 |
| 30 | — | 1 | — | — | 1 | ·1 |
| Total spells | 858 | 900 | 1203 | 591 | 3552 | 3559 |
| Total days | 3248 | 3242 | 4307 | 2097 | 12894 | |
| Average days per spell | 3·78 | 3·60 | 3·58 | 3·55 | 3·63 | |
| | | | | | $x = 0{\cdot}7272$ | $a = 1332$ |

WET

| Length of spell in days | Winter Dec.-Feb. | Spring Mar.-May | Summer Jun.-Sept. | Autumn Oct.-Nov. | 50 years Observed | 50 years Calculated |
|---|---|---|---|---|---|---|
| 1 | 625 | 595 | 827 | 378 | 2425 | 2419 |
| 2 | 170 | 201 | 256 | 131 | 758 | 666 |
| 3 | 39 | 61 | 68 | 35 | 203 | 244 |
| 4 | 16 | 20 | 29 | 27 | 92 | 101 |
| 5 | 13 | 9 | 9 | 7 | 38 | 44·6 |
| 6 | 2 | 2 | 9 | 5 | 18 | 20·4 |
| 7 | — | 1 | 1 | 3 | 5 | 9·7 |
| 8 | 1 | 1 | 1 | 3 | 6 | 4·6 |
| 9 | — | 2 | 1 | — | 3 | 2·3 |
| 10 | — | 1 | — | 2 | 3 | 1·1 |
| 11 | — | — | — | — | — | ·5 |
| 12 | — | — | — | 1 | 1 | ·3 |
| 13 | — | — | — | — | — | ·2 |
| 14 | — | — | — | — | — | ·1 |
| 15 | — | — | — | — | — | ·1 |
| Total spells | 866 | 893 | 1201 | 592 | 3552 | 3514 |
| Total days | 1231 | 1360 | 1782 | 995 | 5368 | |
| Average days per spell | 1·42 | 1·52 | 1·48 | 1·68 | 1·51 | |
| | | | | | $x = 0{\cdot}551$ | $a = 4391$ |

### 2. Dry spells at Moncton

The results of an examination of the rainfall records at Moncton, N.B., during the period 1900-1949 inclusive are shown in Table 1. It will be noted that the average number of days per spell is essentially a constant for the four seasons – with an average of 3·63 days per dry spell and 1·51 days per wet spell.

In order to fit data to a curve of the logarithmic series, the second term must be less than one-half the first, the third term must be less than one-third the first, etc. The data for dry spells at Moncton do not conform to this ; accordingly they cannot be fitted to a ' Williams ' series. A study of the first 16 terms, however, gives an average value of ·7272 for $x$ as a constant ratio between successive terms; (knowing $x$, the value $\alpha = 1332$ can be readily computed). The frequency distribution for dry spells at Moncton is a simple geometric series with the sum (total number of spells) equal to 3552. Utilizing the equations $s = a/(1 - x)$ and $t_n = ax^{n-1}$ where $a$ is the first term and $t_n$ is the $n$th term, the calculated values in Table 1 may be determined.

### 3. Wet spells at Moncton

The data for wet spells at Moncton for the period 1900-1949 inclusive are included in Table 1. Since the preliminary condition is satisfied – namely, the second term is less than one-half the first, etc,. – a curve of the logarithmic series may be fitted to the data. Williams (1947) has produced a chart giving a graphical solution of $x$ (in this case $x = 0{\cdot}551$) if the average number of days per spell is known. He also indicates the method of determining $\alpha$ (in this case $\alpha = 4391$). Using these values of $\alpha$ and $x$, the calculated figures for wet spells in Table 1 can be determined.

### 4. Statistical forecasting of wet and dry spells

As an aid in medium-range forecasting ($\simeq$ 5 days) these figures may have merit. In attempting to ' fix ' the ending date of a wet (or dry) spell which lasts beyond 2 or 2½ days, forecasters at present use a calculated or ' educated ' guess. In the absence of charts (synoptic and thermodynamic), the forecaster should turn to statistics for a *guide*, not for an absolute value. Suitable calculations will give the percentage probability that a 4-day dry spell will stretch out to 7 days, 9 days, etc.

### References


| | | |
|---|---|---|
| Gold, E. | 1929 | *Quart. J. R. Met. Soc.*, **55**, p. 307. |
| Williams, C. B. | 1947 | *J. Ecol.*, **34**, p. 253. |
| | 1952 | *Quart. J. R. Met. Soc.*, **78**, p. 91. |


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551.577.12

# A note on the classification of annual rainfall-distribution types

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(Manuscript received 21 July 1953, in revised form 18 August 1953)

### Summary

This is an attempt to introduce into earlier methods of classifying rainfall-distribution patterns the factor of intensity, by means of three intensity grades – exceptionally wet, normal and exceptionally dry. Several significant distributional features are commented upon, and an attempt is made to correlate these distributions with the frequency of ' weather types.'

## 1. Introduction

Thirty years ago a classification of annual rainfall-distribution types was presented for the whole of the British Isles (Salter and Glasspoole 1923). Whilst the method then used indicated *relative* intensity of rainfall as between east and west, it did not indicate *absolute* intensity, i.e., whether conditions were wetter or drier than normal. It is the attempt to introduce this factor of intensity of development, omitted in the earlier classification, as well as the distributional aspect which was included therein, which forms the major theme of this short paper.

As this is primarily an attempt to formulate and justify methods, only part of Britain has been considered, namely southern England. On the other hand, as large a number of stations as possible has been employed, for a reasonably adequate period of years – 117 stations for the period 1900-1939, with supporting evidence drawn, for areas otherwise scantily covered, from ten stations for the period 1901-1939 and from six stations for that of 1900-1938.

## 2. Intensity of development

To avoid giving an impression of detailed accuracy which, when dealing with such a variable phenomenon as rainfall, is liable to be unjustified, only three grades of intensity are here defined – exceptionally wet, normal and exceptionally dry. In the search for a range of values which may suitably be termed normal, a suggestion from Conrad and Pollak (1950) has largely been adopted, i.e., that the normal should include any occurrence which differs from the median by not more than the quartile deviation. With a frequency distribution following the Gaussian curve the limits of the normal would be the quartile values, and these have been taken even when the distribution is slightly skew. Thus the normal is taken as being the central 50 per cent of the occurrences, and for this reason the median has here been adopted as the mean value.

Consideration was also given to the use of either the mean deviation or the standard deviation as providing a range of values suitable for designation as normal. As both of these are in large measure dependent upon the frequency and magnitude of the larger deviations from the mean, this would have led to the definition of the normal in terms of exceptional conditions, rather than the more logical definition of the exceptional in terms of the normal, as is here employed.

The definition of the exceptional used in this paper is as follows – an exceptionally wet year is taken as occurring at any one station when the rainfall total of that year exceeds the upper quartile, i.e., the upper limit of the normal, by at least ten per cent of the median; conversely, an exceptionally dry year occurs when the annual rainfall total is less than the lower quartile by at least ten per cent of the median. Thus exceptional conditions are related both to the mean value, i.e., the median, and to the limits of the normal, i.e., the quartiles.

## 3. Annual distribution patterns

On this basis 40 annual distribution maps were drawn, showing not only areas experiencing normal and exceptional conditions, but also those transition areas where conditions were wetter or drier than normal but not exceptionally so. Moreover, the areas of maximum intensity are also shown, the critical value being a difference between the annual total and the respective quartile value of at least 20 per cent of the median. Exceptionally wet, exceptionally dry and normal conditions were then considered separately, each set of conditions being classified according to the part of southern England which they affected, and a distinction being drawn between ' major ' distributions, and ' localized ' distributions based on only a few stations (Table 1). This separate areal classification of each intensity grouping means that many years are entered under two categories in Table 1.

In Figs. 1 and 2 several specific examples of annual distributions are shown, and from these and the other annual maps a few general points seem worthy of comment.

### (a) *Exceptionally wet conditions (Fig. 1)*

(i) In western areas these occur either as extremely localized features, or in conjunction with very widespread high rainfall conditions over the whole of the western and central areas (compare Glasspoole 1926); (ii) specifically eastern distributions are relatively frequent and well marked, especially in comparison with specifically western distributions; (iii) the limits of the southern distributions seem greatly influenced by the relief features which parallel the coast as far inland as the Marlborough and the North Downs.

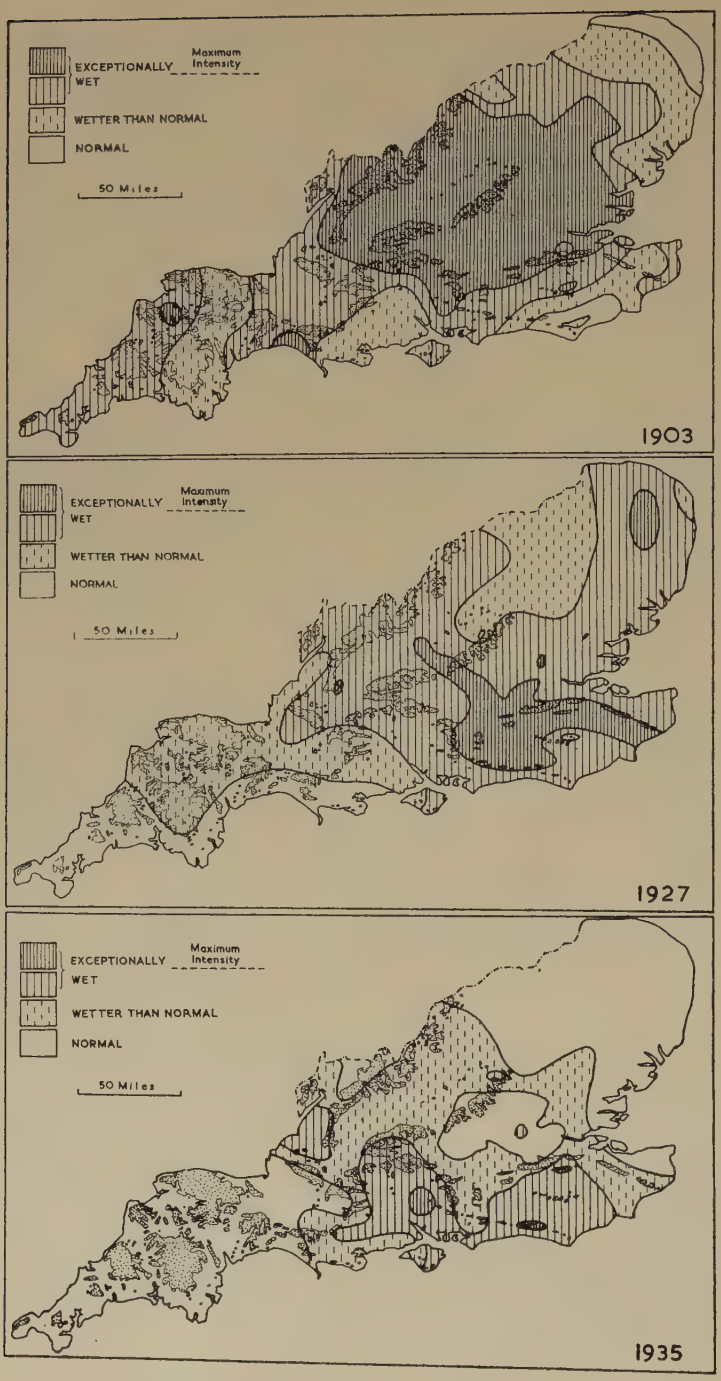


Figure 1. Examples of exceptionally high rainfall annual distribution types (data were taken from *British Rainfall*). 1903 – central and western distribution; 1927 – eastern distribution; 1935 – southern distribution. Land above 500 ft O.D. stippled. The terms ' normal ' and ' exceptional ' are used as defined in Section 2 under ' Intensity of development.'

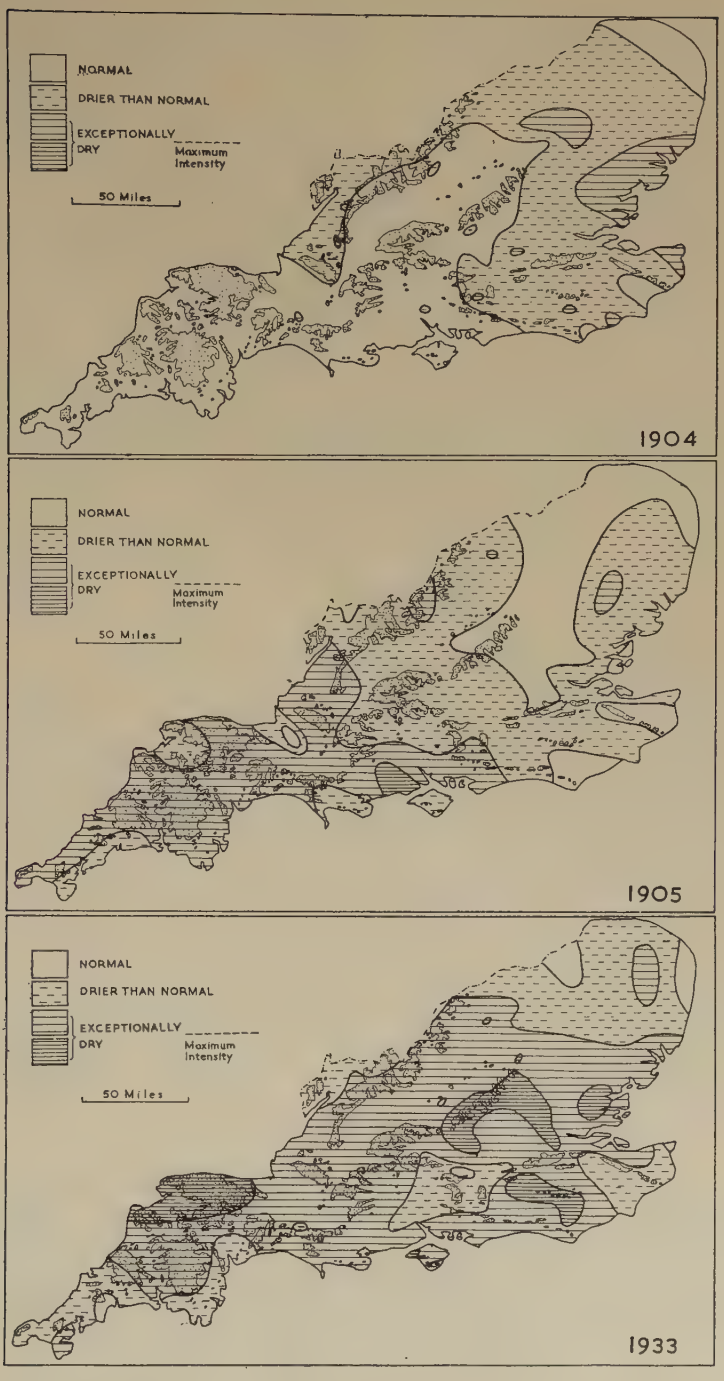


Figure 2. Examples of exceptionally low rainfall annual distribution types (data were taken from *British Rainfall*). 1904 – eastern distribution; 1905 – western distribution; 1933 – central distribution. Land above 500 ft O.D. stippled. The terms ' normal ' and ' exceptional ' are used as defined in Section 2 under ' Intensity of development.'

TABLE 1. CLASSIFICATION OF ANNUAL RAINFALL-DISTRIBUTION TYPES OVER SOUTHERN ENGLAND

| Year | Exceptionally wet | Exceptionally dry | Normal | Year | Exceptionally wet | Exceptionally dry | Normal |
|---|---|---|---|---|---|---|---|
| 1900 | | | C | 1920 | | (e) | C |
| 1901 | | C | | 1921 | | C | |
| 1902 | | C | | 1922 | | | C |
| 1903 | C and W | | | 1923 | (s) and (w) | | C |
| 1904 | | E | W | 1924 | C and W | | |
| 1905 | | W | E | 1925 | (s) | | C |
| 1906 | | (w) | C | 1926 | | (w) | C |
| 1907 | | (e) | C | 1927 | E | | W |
| 1908 | | W | E | 1928 | (s) and (w) | | C |
| 1909 | (e) | (w) | C | 1929 | | (e) | C |
| 1910 | (w) | | E | 1930 | (w) | | C |
| 1911 | | (n) | C | 1931 | | | C |
| 1912 | C and W | | E | 1932 | | | C |
| 1913 | | | C | 1933 | | C | |
| 1914 | (s) | | C | 1934 | | N | S |
| 1915 | S | | W | 1935 | S | | W and E |
| 1916 | E | | W | 1936 | | | C |
| 1917 | | (w) | C | 1937 | E | | W |
| 1918 | (e) | | C | 1938 | | E | W |
| 1919 | | | C | 1939 | E | | W |

Capitals indicate major distributions; lower case in brackets indicate localized distributions.

W = distribution centred over the western part of S. England.

E = distribution centred over the eastern part of S. England.

S = distribution centred over the southern part of S. England, extending inland from the Channel coast.

N = distribution extending into S. England from across the northern boundary of the area considered.

C = distribution centred over the central part, and often extending over the whole or nearly the whole, of S. England (the wider sense applies especially to normal conditions).

### (b) *Exceptionally dry conditions (Fig. 2)*

(i) These are frequently characterized by scattered areas of exceptional conditions, embedded in a matrix of large areas of transitional characteristics; (ii) specifically western distributions are more frequent and marked than in the case of high rainfall; (iii) maximum intensity and areal extent occur with centrally-situated distributions, as in 1933 and especially in 1921 (Brooks and Glasspoole 1922).

### (c) *Normal conditions (Figs. 1 and 2)*

(i) In 21 out of the 40 years these occurred over nearly all of southern England, and 13 of these years fell in the 16-year period 1917-1932; (ii) in six years normal conditions were either non-existent over southern England as in 1921, or of negligible extent as in 1903 (Fig. 1) and 1933 (Fig. 2); (iii) in the remaining years the areas experiencing normal conditions were merely those parts unaffected by exceptional or almost exceptional conditions, and were thus rather difficult to classify (see Table 1; 1927 and 1935 in Fig. 1; 1904 and 1905 in Fig. 2).

## 4. CAUSATION

General relationships between rainfall-distribution patterns and mean pressure anomalies have already received consideration (Salter and Glasspoole 1923; Brooks 1926); the possibilities of correlations with weather types, as recently defined by Levick (1949) and Lamb (1950), nevertheless deserve a brief review.

TABLE 2. EXAMPLES OF PERCENTAGE FREQUENCIES OF WEATHER TYPES IN YEARS OF NORMAL RAINFALL VALUES

| Year | AC. | E. | N. | NW. | W. or SW. | S. | C. |
|---|---|---|---|---|---|---|---|
| | % | % | % | % | % | % | % |
| 1900 | 20 | 9 | 8 | 9 | 43 | 11 | 20 |
| 1919 | 28 | 12 | 15 | 6 | 32 | 7 | 21 |
| 1932 | 34 | 6 | 19 | 8 | 28 | 5 | 21 |

Data were taken from Levick (1949) for six weather types – anticyclonic (AC.), easterly (E.), northerly (N.), north-westerly (NW.), westerly or south-westerly (W. or SW.) and southerly (S.). For cyclonic (C.) conditions data were supplied from unpublished work by H. H. Lamb. As these represent a reclassification of certain days, cyclonic percentage values are not included with those for the other six types in the above table.

The three years with most widespread normal conditions – 1900, 1919 and 1932 – provide a suitable example (Table 2). An apparently uniform characteristic is the percentage frequency of cyclonic conditions, but similar values obtained in the dry years 1901, 1902 and 1905, and in the wet year 1939. Comparable discrepancies were found in all cases for individual weather types, and for all combinations of the various types. Weather types seem, in fact, too rough a tool for this task especially as comparable precipitation values cannot be expected with every occurrence of a given weather type. There is, in fact, no ready means of relating intensity of development of weather types with their frequency of occurrence.

## 5. Conclusions

It would seem that for classifying rainfall-distribution types this method of first establishing a series of intensity grades leads to a fuller, if more complex, picture. The criteria used for the definition of intensity are based on both practical and theoretical considerations, and within the limits of this review they have worked satisfactorily.

The resultant distribution patterns would appear to indicate that, with regard to the year as a whole, specifically eastern distributions are more frequent and extensive than are specifically western ones in the case of exceptionally high rainfall conditions, whilst the reverse holds true for exceptionally low rainfall conditions. The extension of centrally-situated distributions to affect either eastern or western areas (see 1903, Fig. 1, and 1933, Fig. 2) may modify this relationship, however, whilst investigation by seasons, or by a longer term of years, is needed before this can be established. The finding may nevertheless be of some interest in relation to our water resources.

From Section 3 (c), the relatively reliable character of our annual rainfall appears to emerge; having regard to evaporation, however, it might be worth extending this method to an analysis of the distribution of rainfall by 'summer' and 'winter' half-years.

The answer to the problems of causation would seem to lie at least as much in the intensity of development of meteorological systems as in their frequency of occurrence. This can only be approached through a study of upper-air conditions, possibly along the lines already pioneered by Belasco (1945, 1952).

## Acknowledgments

My thanks are due to Professor Wilfred Smith of Liverpool University for comments and advice, and to Mr. H. H. Lamb of the Meteorological Office for the provision of unpublished data concerning the frequency of certain weather types.

## References

Belasco, J. E. 1945 *Quart. J. R. Met. Soc.*, **71**, p. 351.
1952 *Geophys. Mem. Met. Off.*, London, No. 87.
Brooks, C. E. P. 1926 *Quart. J. R. Met. Soc.*, **52**, p. 387.
Brooks, C. E. P. and Glasspoole, J. 1922 *Ibid.*, **48**, p. 139.
Conrad, V. and Pollak, L. W. 1950 *Methods in Climatology*, Harvard University Press, p. 47.
Glasspoole, J. 1926 *Quart. J. R. Met. Soc.*, **52**, p. 363.
Lamb, H. H. 1950 *Ibid.*, **76**, p. 393.
Levick, R. B. M. 1949 *Weather*, **4**, p. 206.
Salter, M. de C. S. and Glasspoole, J. 1923 *Quart. J. R. Met. Soc.*, **49**, p. 207.

551.509.317

# On wind analysis in low latitudes

By J. R. SCOTT

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(Manuscript received 10 March 1953, in revised form 8 June 1953)

One of the more formidable tasks of the forecaster near the equator is the quantitative interpolation between single or comparatively isolated groups of wind observations on the constant-level wind chart. The most satisfying method yet described, from the theoretical standpoint, is that used by Forsdyke (1949) as the preliminary to computing isopleths of horizontal divergence. This method is, however, tedious to use as a routine and provides no mechanism for smoothing the observations. Another method involves the construction of streamlines and isotachs (as described for example by Palmer 1952). This method, though in theory providing a quantitative picture of the wind field, is in practice much less objective than that used by Forsdyke. The isotachs must be drawn after reference to the streamline field, and this latter may be constructed in a large variety of significantly different ways, dependent on the conception of the flow pattern which the analyst has before commencing his analysis, i.e., the ' model ' he has in mind. This method of analysis does introduce a process of smoothing the observations, but they are smoothed to agree with the analyst's pre-conception. The smoothing does not emerge from the analysis itself. The diagrams published by Ramage (1951) and Thompson (1951) illustrate this type of smoothing as well as two of the types of pre-conception used in wind analysis. The disadvantages of the methods mentioned above led the writer to investigate the method described below.

In middle latitudes a non-divergent streamline pattern adequately represents air flow above the layer significantly influenced by surface effects, and there appears no reason to suppose that a similar pattern could not perform the same service in low latitudes. Some numerical support was given to this view by the following consideration of purely divergent fields of flow. Consider two air streams converging normally at 5 kt into a strip of finite width. The average divergences in the strip are $-10^{-4}$ sec$^{-1}$ and $-10^{-5}$ sec$^{-1}$ when its widths are respectively 28 and 280 n. mi. Now Kindle (quoted by Forsdyke 1949) has associated divergences of $-10^{-4}$ sec$^{-1}$, and $-10^{-5}$ to $-10^{-6}$ sec$^{-1}$, with heavy and light rain respectively. Accepting these figures it will be seen that a non-divergent flow pattern could represent, with no more than 5 kt vector error at any point, the wind field in the vicinity of a belt of heavy rain 28 n. mi wide, or a belt of light to moderate rain some 280 n. mi wide. A similar consideration of circular rain areas shows that an inward radial drift of 5 kt through the perimeter is enough to account for circular areas of heavy and light rain of about 0·75 and 75 one-degree squares respectively, or for a circular rain area of 32 one-degree squares of which 1·6 are of heavy and the rest of light rain. Experience suggests that, except in tropical cyclones, widespread heavy rain is rare. It does appear in narrow zones, locally within larger rain areas and as isolated storms which do not appear to approach the extent of 0·75 one-degree squares. It is therefore not unreasonable to suppose that there does exist on most occasions a non-divergent flow pattern which would represent quantitatively the horizontal field of flow within limits reasonable for the purpose of wind estimation.

The only available method in low latitudes for finding the non-divergent flow pattern is by analysis of the wind observations themselves, and the author has analysed more than 60 charts of winds at 5,000 ft and 10,000 ft into such patterns (it was soon realized that surface effects prevented useful analysis of winds at lower levels). In brief, the technique was the same as would be an attempt to construct the pattern of surface isobars in middle latitudes given 2,000-ft wind observations only. The drawing of the charts was started by inserting short streamlines spaced, with a scale, at distances varying inversely with speed, in areas where observations existed. This process gave an idea of the number of lines to be dealt with, and lines were next tentatively joined across areas where no observations existed. It was often necessary to make several trials before obtaining a satisfactorily balanced pattern – a pattern in which all non-suspect observations were quantitatively represented within reasonable limits. There were no occasions on which a satisfactory pattern was not obtained.

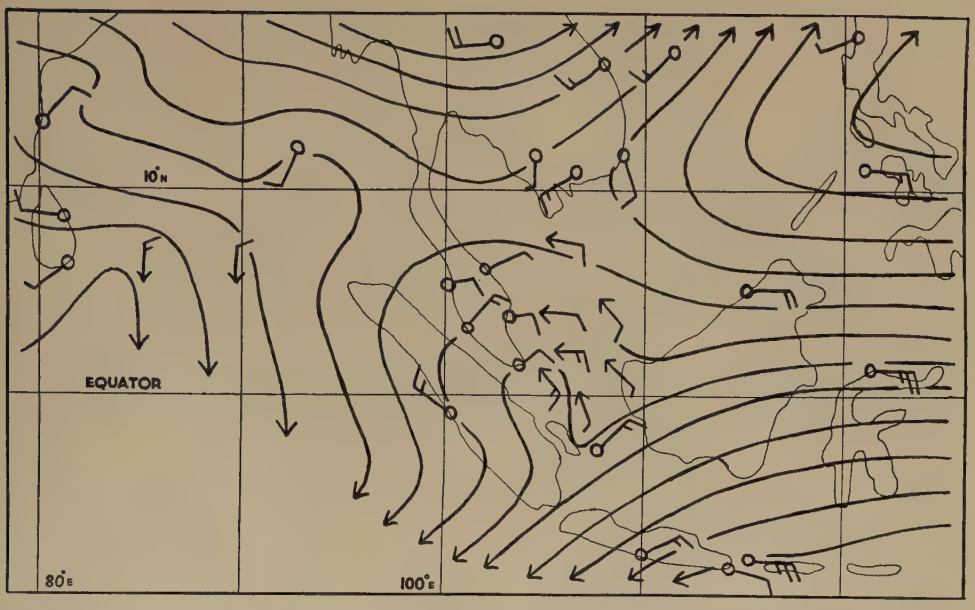


Figure 1. Analysis of 10,000-ft wind observations of 12 July 1951. A full flèche indicates 10 kt. Circles denote pilot-balloon ascents and arrow heads, aircraft observations.

Fig. 1 is a reproduction of one of the charts analysed. It is of additional interest because its predecessors gave indication that the anticlockwise circulation south of Malaya (which was associated with a large rain area) developed from a wave-like distortion of the streamlines which moved westwards from Borneo.

## References


| | | |
|---|---|---|
| Forsdyke, A. G. | 1949 | *Geophys. Mem. Met. Off.*, London, No. 82, Pp. 21 and 22. |
| Palmer, C. E. | 1952 | *Quart. J. R. Met. Soc.*, **78**, p. 126 *et seq.* |
| Ramage, C. S. | 1951 | *J. Met.*, **8**, p. 289 *et seq.* |
| Thompson, B. W. | 1951 | *Quart. J. R. Met. Soc.*, **77**, p. 577 *et seq.* |


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551.558.21

# A lee-wave scale

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(Manuscript received 22 May 1953, in revised form 31 August 1953)

Although no completely proven method of forecasting the occurrence of lee waves has yet been devised, comparatively recent investigation into the nature of these waves has at least suggested a technique which may be usefully employed until a more advanced mathematical analysis is possible.

In a notable contribution to lee-wave theory Scorer (1949) has shown that the occurrence, and to some extent the character, of waves in an air stream depend on the variation with height of a certain parameter which, denoted by $l$, is given by

$$l^2 = \left(\frac{B}{U}\right)^2 - \frac{1}{U}\frac{\partial^2 U}{\partial z^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$B^2 = \frac{g}{\theta}\frac{\partial \theta}{\partial z} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where $g$ is the acceleration due to gravity, $U$ is the wind speed, $z$ is the height and $\theta$ is the potential temperature.

Since its exact calculation would be far too laborious for routine procedure it is proposed to show a convenient, practical method of deriving this important parameter.

Fortunately the second term on the right of Eq. (1) is usually small enough to be neglected. This is not always the case, but since there is no speedy method of evaluating this awkward second partial derivative we are obliged to omit the exceptional cases. The omission is not serious and at least reduces the problem to the determination of $B/U$.

This is not difficult, for if $T$, $p$, and $\phi$ are the absolute temperature, pressure and entropy respectively, at a height $z$ above some standard pressure level $p_0$, then

$$\frac{1}{\theta}\frac{\partial \theta}{\partial z} = \frac{1}{T}\left(\frac{\partial T}{\partial z} + \Gamma\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

and for an isothermal column of air

$$z = \frac{RT}{g}\ln\left(\frac{p_0}{p}\right) = \frac{T}{\Gamma}\left(\frac{\phi\Gamma}{gA} - \ln T\right) \quad . \quad . \quad . \quad . \quad (4)$$

where $A$ is the reciprocal of the mechanical equivalent of heat, $R$ is the appropriate gas constant and $\Gamma$ is the dry-adiabatic lapse rate.

So that when, as in Fig. 1, the lapse rate at a point $X$ on the tephigram is represented as a straight line $XY$ in a small height interval we have, to the first order of small quantities,

$$\frac{1}{\theta}\frac{\partial \theta}{\partial z} = \frac{1}{T}\left(\frac{T_N - T_M}{z_Y - z_X}\right) \quad . \quad . \quad . \quad . \quad . \quad (5)$$

and

$$z_Y - z_X = z_L - z_X = \frac{T}{gA}(\phi_L - \phi_X) \quad . \quad . \quad . \quad (6)$$

where the subscripts refer to the points considered.

Since the lengths $LX$ and $NM$ are measures of $\phi_L - \phi_X$ and $T_N - T_M$ respectively we can write

$$B^2 = \frac{g^2 A}{T^2}\frac{NM}{LX}k \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

$k$ being a constant appropriate to the scale of tephigram used.

If the angle $XMY$ is assumed constant and in particular equal to 45°, then,

$$B^2 = \frac{g^2 A}{T^2\sqrt{2}}\frac{MY}{MX}k \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

Construction of a triangle $XPQ$ similar to $XMY$ such that $PX$ is proportional to $T^{-2}$ now leads to

$$B^2 = \frac{g^2 A}{\sqrt{2}}PQ\,k \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

Since the height interval $z_Y - z_X$ can be made as small as we please, the only error involved in this relationship is due to the assumption that $PQ$ is parallel to the isobar through $Y$. But this error is usually small enough to be neglected.

Hence a scale as shown in Fig. 2 can be constructed. Temperature (°F) is indicated by an inverse-square scale of absolute temperature along $PH$, while $B$ is measured along $PK$. If 10 cm is chosen to represent 300°K on the temperature scale then the graduations on the $PK$ scale applicable to the Meteorological Office tephigrams on Form 2810 are at distances 0·00367 $B^2$ cm from $P$; $B$ being given in $hr^{-1}$.

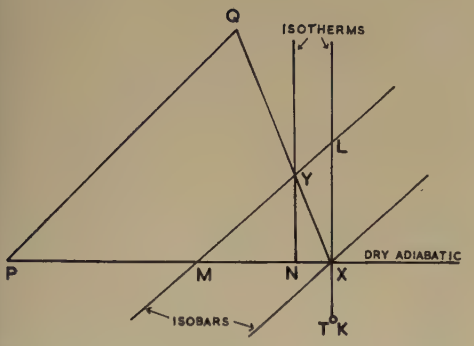


Figure 1. The short straight line XY represents the lapse rate at the point X.

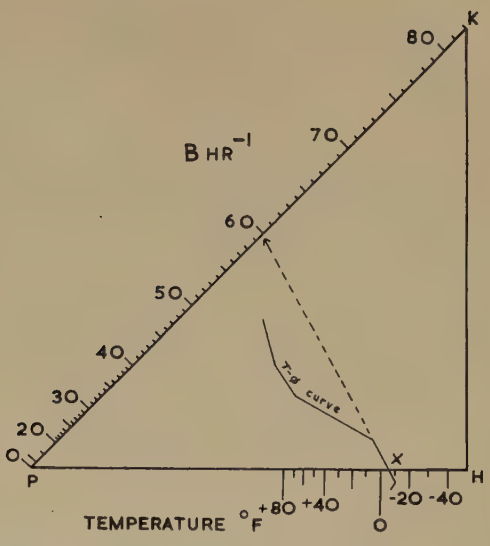


Figure 2. The example shows that B for the point X at $-6°$F on the given $T-\phi$ curve is 60 $hr^{-1}$.

To find the value of $B$ at a particular point, say $X$, of a given $T$-$\phi$ curve the scale must be laid on the tephigram so that $PH$ is parallel to the dry adiabatics with the appropriate temperature on $X$. A straight line to represent the lapse rate at $X$ is then produced to meet $PK$ where the required value can be read. The method is illustrated in Fig. 2. Division by $U$ then yields the approximate value of $l$.

Papers by Scorer (1953) give details of the conclusions to be drawn from the vertical variation of $l$. Broadly speaking, the most important criterion appears to be that a decrease of $l$ with height in the particular layers considered favours the occurrence of lee waves. In special circumstances an estimate of the possible wavelengths can be attempted but much more investigation is necessary before further deductions can be made with reasonable confidence.

It does seem certain, however, that $l$ is a fundamental parameter associated with wave phenomena and that its behaviour in observed conditions merits careful study. Hence the need for a quick, practical method of deriving its value from the temperature and wind data usually available.

Acknowledgment

I am indebted to the Director of the Meteorological Office for permission to publish this note.

References

Scorer, R. S. 1949 *Quart. J. R. Met. Soc.*, **75**, p. 41.
1953 *Ibid.*, **79**, p. 70.
1953 *Gliding*, **4**, p. 32.

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551.521.12 (595)

# Solar radiation at Singapore

By C. G. WEBB

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(Manuscript received 22 August 1953)

Continuous recording of the solar radiation falling on a horizontal surface has been in progress at Singapore since June 1952, using a Kipp solarimetric thermopile installed in an unobstructed position in 1°19′N, 103°49′E, at a height of 120 ft above m.s.l.

During the year from 1 Aug. 1952 to 31 July 1953, 347 daily records were obtained, of which 323 were unbroken. The corrected monthly and annual totals, based on the sums of the means for each hour during the month, and the totals on extreme days, were as follows, in cal $cm^{-2}$.

TABLE 1.

| Month | Sum of hourly means | Total during month | Totals on extreme days | |
|---|---|---|---|---|
| 1952 | | | | |
| Aug. | 314 | 9,734 | 500 | 98 |
| Sept. | 466 | 13,980 | 642 | 197 |
| Oct. | 468 | 14,508 | 701 | 236 |
| Nov. | 357 | 10,710 | 593 | 90 |
| Dec. | 402 | 12,462 | 599 | 117 |
| 1953 | | | | |
| Jan. | 374 | 11,594 | 544 | 161 |
| Feb. | 385 | 10,780 | 601 | 191 |
| Mar. | 385 | 11,935 | 620 | 233 |
| Apr. | 378 | 11,340 | 540 | 140 |
| May | 355 | 11,005 | 542 | 126 |
| June | 373 | 11,010 | 487 | 194 |
| July | 322 | 9,982 | 524 | 107 |
| Year 1952-3 | 381 | 139,040 | 701 | 90 |
| Range | 146 | | 214 | 146 |
| Range (% of mean) | ± 19 | | ± 19 | ± 46 |

Radiation tended to exceed average near the equinoxes and solstices, but the only marked variation appears to be the occurrence of a peak in Oct. 1952. A 30-day variation was noticed, which agreed in certain respects with van de Stok's observations of the lunar variation of cloud over Java (' The influence of the moon on the cloudiness of the sky,' *Observations at the Magnetic and Meteorological Observatory at Batavia*, VI, Appendix I, 1885). There was a large irregular day-to-day variation which, however, was insufficient to mask the above-mentioned effect, and it is thought that the latter may be a significant meteorological factor at very low latitudes.

551.573

# Notice

## Evaporation records at Camden Square

Evaporation from a free-water surface in a tank 6 ft square has been recorded daily at 62 Camden Square, N.W.1, since 1885. The observations were started by the late G. J. Symons. A summary of the readings has been published in the annual volumes of *British Rainfall* and five-day means throughout the year for the period 1905-1918 were compared with similar values for the duration of sunshine, wind movement, water temperature and soil temperature in *British Rainfall*, Pp. 6-13, 1919.

From many aspects it is desirable to continue such long records where they can be maintained under unchanging conditions. It was found, however, that the tank was beginning to wear out and that there were serious risks of a leak. Moreover, other records are now available, including that at Kew Observatory. With this additional information it is apparent that the record is only representative of the evaporation from a free-water surface in a relatively sheltered garden. After careful consideration the British Rainfall Organization Fund Committee of the Royal Meteorological Society decided that this long record had now served its useful purpose, and it was recommended that the record should be allowed to terminate in December 1955, if no weighty representations are made for its continuance. This recommendation was accepted by Council.

J. Glasspoole

# Correspondence

551.574.1

## The collection efficiency of ice crystals

By I. C. Browne

In his communicated reply to the discussion on his paper 'Theory of the radar upper band' (*Q.J.*, **79**, p. 444), Dr. Wexler asks on what physical basis I suggest that a collection efficiency $E$ for falling plates of the order of 0·1 would be more valid than the value of 0·8 which he assumed. In justification of his assumption he quotes some results obtained by Langmuir.

Dr. Wexler appears to err in his application of Langmuir's work, in that he has taken the flow to be aerodynamic or potential, rather than viscous. According to Langmuir (*Project Cirrus Occasional Report*, No. 1, Schenectady, 1948), the transition from viscous to aerodynamic flow for falling spheres occurs at a Reynolds number of the order of 60. It seems likely that the critical Reynolds number for falling plates will be of the same order of magnitude. In the present case, plate-like crystals of radius 200 $\mu$ are supposed to fall at 35 cm $sec^{-1}$ in air at a height of about 5 km and a temperature of about − 15°C. Under these conditions, the Reynolds number is equal to 6, and so Langmuir's calculations for viscous flow should have been used, leading to a value for the collection efficiency $E$ for spheres of 0·2. For the reasons given by Dr. Wexler, the corresponding value of $E$ for flat plates may be about 30 per cent greater, though this is not certain.

It should be noted that the collection efficiency in this range of values decreases rapidly with increasing particle radius $S$ and with decreasing cloud-droplet radius $r$, other quantities being assumed the same. For example, $E$ falls to 0·1 if $S$ is increased to 260 $\mu$ or if $r$ is decreased from 10 $\mu$ to 8·8 $\mu$, while if $S$ is increased to 350 $\mu$, or $r$ decreased to 7·5 $\mu$, $E$ falls to zero. Thus the exact values assumed for the radius of the crystals and of the surrounding cloud droplets are critical.

Probably the issue can be settled satisfactorily only by thorough experiments on the collection efficiency of discs under various conditions.

University of Manchester, Jodrell Bank Experimental Station,
Holmes Chapel,
Crewe, Cheshire.
24 July 1953.

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## Reply

By R. Wexler

The collection efficiency was computed from potential flow in preference to viscous flow. There is considerable question as to whether Langmuir was justified in using the concept of viscous flow for computing collection efficiency, which appears to depend on the flow pattern at a relatively long distance from the falling particle. According to Goldstein (*Modern developments in fluid dynamics*, 1938, p. 492), the inertia terms become more important than the viscous terms at a great distance from a falling sphere even at low Reynolds numbers. The opinion preferring potential flow for the purpose of computing collection efficiency was first expressed to the writer by Professor P. A. Sheppard.

Further, Langmuir's computations for viscous flow assumed the cloud drops to be points, whereas actually the cloud drops at low Reynolds numbers are of comparative size to that of the falling sphere. As a result his values of collection efficiency for viscous flow are much too small.

Dr. Browne appears reluctant to admit the existence of graupel, which could hardly form if the collection efficiency of ice crystals were negligible, as would be inferred from his calculations. Observations of small ice crystals with numerous frozen cloud drops from winter clouds of relatively low liquid content favour high values of collection efficiency.

Weather Radar Research, M.I.T.,
Cambridge, Mass.,
U.S.A.
5 September 1953.

551.558

## A new method for measuring vertical air currents

By P. Barratt and I. C. Browne

The measurement of vertical motions in the atmosphere has great meteorological interest. So far, most methods have involved the use of powered aircraft, gliders or balloons, but these methods have the disadvantage that measurements can be made in only one place at a time. Warner and Bowen (1953) have deduced vertical winds by measuring with radar the change in height of strips of metal foil dropped from an aircraft, but at present this technique cannot be used for measurements in rain clouds.

When a radar beam is directed vertically into a raincloud, the frequency of the echo from each raindrop is changed by a small amount, the Doppler shift, depending on the velocity of the drop relative to that of the radar. This effect produces fluctuations in the intensity of the echo arising from many drops moving relative to one another, and measurements of these fluctuations can yield information about the drop-size distribution and the turbulence in the cloud (Hilst 1949, Browne 1952). If the drops possess a mean velocity relative to the radar, there will also be a mean Doppler shift in the frequency of the returned echo. This could, in principle, be measured by comparing the frequency of the echo with that of a standard signal generator, but to do this, the frequency of the radar transmitter and receiver must be stabilised to better than one part in $10^8$. The frequency stability of the conventional centimetric radar is rarely better than one part in $10^5$. Improvement is both elaborate and costly.

A technique is being developed at Cambridge in which the difficulty of obtaining frequency stability is avoided in a simple way by comparing the frequency of the cloud echo with that of an echo from a stationary target at the same range. The beat frequency between these echoes is a direct measure of the mean Doppler shift, and is unaffected by changes in the radar frequency or in the radar phase. The method used is to split the radar beam emerging from the vertically-directed aerial by means of a flat metal plate, inclined at 45°, fixed above the aerial. A fraction of the radiated power is thus directed towards a suitable ground target, the remainder passing upwards to the cloud being investigated. The system is analogous to a Michelson split-beam interferometer in which one mirror is fixed and the other moving.

Some preliminary measurements with this device using a radar working on a frequency of 3,300 Mc/sec were obtained on 27 May 1953, during a shower when the precipitation rate was 10 mm $hr^{-1}$. Using Best's data for drop-size distribution (Best 1950), the power spectrum was calculated which would have been expected in the absence of vertical draughts. Although the forms of the observed and calculated power spectra agreed, the frequency of maximum power return was measured to be greater than that expected by approximately 50 c/sec. This corresponds to a velocity of about 2 m $sec^{-1}$ and is interpreted as indicating a downdraught of this order at a height of 5,000 ft, the height for which the measurements were made.

The technique could be extended to measurements in cloudless air by examining the echo returned from reflecting foil dropped into the radar beam. Details of the method and measurements will be published in full shortly.

Cavendish Laboratory, Cambridge.
University of Manchester,
Jodrell Bank Experimental Station.
18 September 1953.

### References


| | | |
|---|---|---|
| Best, A. C. | 1950 | *Quart. J. R. Met. Soc.*, **76**, p. 16. |
| Browne, I. C. | 1952 | Dissertation, Cambridge University. |
| Hilst, G. R. | 1949 | *M.I.T. Weather Radar Res. Tech. Rep.*, No. 9A. |
| Warner, J. and Bowen, E. G. | 1953 | *Tellus*, **5**, p. 36. |

551.594.221

# Lightning

By T. W. WORMELL

(Symons Memorial Lecture, delivered 29 April 1953. See p. 474 of this issue)

## Discussion

Dr. W. Schaffer : The lecturer has explained that atmospherics from storms more than a certain distance to the south-west have a different waveform, but said it was not known whether this was due to a difference in the nature of the storms or to some effect on transmission by the intervening atmosphere. Has there been any co-ordination with American observations on this question ?

Mr. P. J. Feteris : You gave a list showing the percentages of cloud-to-earth flashes, cloud-to-cloud flashes, etc., deduced from records of field-intensity changes over a period of many years. With the aid of this and other observations a general picture of the charge distribution in a thunderstorm has been built up. However, after five years' experience in following thunderstorm cells with visual and photographic observations of lightning from a dense network of observers I found that there are thunderstorms which give during their life nothing else but cloud-to-earth flashes and short peals of thunder. Others produce only cloud-to-cloud flashes and long thunderclaps. There are all kind of variations in between. In storms with only cloud-to-earth flashes branching of flashes is uncommon. Have any attempts been made to classify storms in this way ? Do flashes of a certain kind occur in groups in the records ? Has branching of flashes anything to do with polarity ?

I think that it is very important to classify the storms and to consider each storm, or better each cell which produces flashes apart, in order to get a more realistic picture which helps us to understand the various mechanisms of charge generation.

Dr. K. R. Ramanathan : In Dr. Wormell's graph showing repeated reflections of lightning discharge pulses from the ionosphere, it was interesting to see as many as 35-40 reflections. This implies a very high reflection coefficient of the ionospheric layer responsible for the reflections and also very small absorption of the radio waves below. This is perhaps true at night; during daytime, the reflections may be expected to be much fewer. Could Dr. Wormell tell us of his experience ?

Dr. J. M. Stagg : This has been a most interesting and instructive lecture. We now have a clear picture of the lightning discharge as observed on the ground. Without wishing to entice Dr. Wormell on to the much-debated ground of the mechanism by which the electric charges are separated, I would be pleased to hear his views on how each of the main clusters of charge, positive and negative, is held sufficiently concentrated in its own part of the cloud to give discharge currents of the size he has described. Mutual repulsion of the constituent charges within each cluster should lead to very diffuse distributions.

Mr. E. Gold : I should like to ask Dr. Wormell if his single station is able to determine the direction as well as the distance of the lightning flash.

Dr. T. W. Wormell (*in reply*) : In reply to Dr. Schaffer, it is certainly very desirable that a search be made, in other parts of the world, for systematic differences in the observed waveforms such as are found in this country, at night, for disturbances originating from distant sources to the south-west and south-east respectively; a site near the eastern seaboard of the United States would be particularly suitable. As far as I am aware, no definite information is yet available on this point for any area other than north-west Europe.

Mr. Feteris raises a number of points. With regard to his description of different types of storms, I would emphasize that caution is necessary in interpreting the results of visual and photographic observations of lightning since many cloud flashes are invisible from the ground and, even in the case of a flash to earth, only the lower part of the flash may be clearly seen. Thus an apparent absence of branching may mean, simply, that the branches were hidden by the lower part of the cloud or by heavy precipitation. Electric-field observations avoid these difficulties but their interpretation may be ambiguous, and in the matter of branching, difficult.

It is certainly true that there are great differences in the relative numbers of different types of flashes in different storms and, indeed, in different stages of the same storm. The behaviour

is so complicated that it is difficult to generalize and I know of no clear-cut classification such as Mr. Feteris wants. In 1939, I suggested a fairly typical behaviour during the lifetime of what we should now call a ' cell ' and Workman and Reynolds have more recently discussed this in more detail (see this *Journal*, **79**, p. 19, 1953).

The great majority of flashes to earth bring down negative charge and are branched, if at all, downwards, like the roots of a tree (we know very little in detail about the rarer flash which brings positive charge to earth). The branches are formed during the progression to earth of the initial leader and their occurrence and position appear to depend on the distribution of electrification in the region between the origin of the leader and the ground. Many factors may play a part in determining this distribution; the height of origin of the leader, the intensity and distribution of precipitation, the distribution of air currents and even the nature of the ground surface, vegetation, etc., may all be significant.

In reply to Dr. Ramanathan, the waveforms showing numerous reflections from the ionosphere were, of course, all observed at night; they came in general from a direction between south and east. Daytime waveforms show comparatively few peaks, approximating rather in appearance to a damped, smooth oscillation, and the reflection picture is here of limited validity. When observing atmospherics from a storm over south-eastern Europe or the Mediterranean region a marked change in the appearance of the waveforms occurs near sunset and the number of peaks increases. When the whole path of propagation, at ionospheric levels (85 km say), has been in shadow for at least two hours, waveforms showing very long trains of echoes are common, particularly if the sensitivity of recording is adjusted so that the initial part of the waveform overshoots the picture. As Dr. Ramanathan emphasizes, this implies a high reflection coefficient but the matter is complicated; the amplitude usually falls off fairly rapidly in the initial part of the waveform and it is only amongst the higher-order peaks, where the angle of incidence is comparatively small, that the diminution of amplitude becomes very slow.

It is difficult to discuss Dr. Stagg's question briefly. The charges are presumably attached, in the main, either to precipitation particles or to cloud particles. The forces on these particles, due to their electric charges, may be comparable with the weights of the particles and the rate at which the boundaries of a charge cluster extend in any direction may be of the order of the velocity of fall of the larger charge-carrying particles. On the other hand, particles near the boundary are being discharged by the continuous ionization currents, which flow into the thundercloud quite apart from lightning discharges, and thus the outer parts of the charged volume are continually being eroded. The conventional idealized picture of the charge distribution in a thundercloud exaggerates, of course, the sharpness and definiteness of the regions concerned.

In reply to Mr. Gold, observations with a pair of mutually perpendicular loop aerials define the vertical plane containing the direction to the source of an atmospheric, and simultaneous observations with a vertical aerial determine which of the two possible directions is the correct one. If, therefore, an analysis of the waveform gives a reliable value for the distance, the source is located by observations from a single station.

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551.524.33 (411)

## Annual recurrences in Edinburgh temperature

By D. H. McINTOSH

(Read 20 May 1953. See *Q.J.*, **79**, p. 262)

Mr. E. Gold : I think that perhaps the most important result of Mr. McIntosh's exhaustive analysis of this long series of Edinburgh temperatures is that the irregularities in the annual curve are not purely chance. Consequently the possibility of forecasting them is not ruled out as it would be if they were purely chance. One of these irregularities came a week ago – Monday, Tuesday and Wednesday of last week were St. Mamertus, St. Pancras and St. Gervais, the Ice Saints, and this year those three nights were nights of ground frost. It may be that this irregularity, which comes frequently, though not invariably, in the first half of May, is more marked in the minimum night temperature than in the mean temperature. It might be worth while to examine the series of minimum temperatures (and indeed also of maximum temperatures) in respect of the irregular changes and the dates of their occurrence.

Dr. R. C. Sutcliffe : We must welcome Mr. McIntosh's attack on the problem of the reality of singularities by the application of objective statistical methods and I do not doubt that the results claimed are valid and helpful. It is however necessary when interpreting statistical results in physical language to keep in mind exactly what has in fact been proved numerically and not to go beyond this. As Professor Weickmann is reported to have said at a meeting in Germany, some people believe in singularities, others do not. I think that disagreements in beliefs amongst scientists are often to be traced to obscurities in language and until we define precisely what we mean by a singularity we cannot hope to get agreement on its reality. So long as the singularity is hydra-headed it will grow new heads when one is struck off.

It is clear that Mr. McIntosh is concerned only to decide whether the trend of annual mean temperature has irregularities dated by the calendar – he is attacking only one head of the monster and the conclusion is that if this is alive at all it is not very vigorous. However, to keep directly to the facts, he has only inquired whether the annual curve departs significantly from his two-term harmonic approximation and, while this approximation is a close fit, its shortest period is six months and we are not certain whether the significant departures are due to rather long-period departures or to short-period effects dated to a few days. There is no reason whatever to suppose that the annual trend of mean temperature should fit a harmonic analysis and the fact that the higher harmonics have uncertain phase and small amplitude may merely mean that the Fourier series converges very slowly. If one examines Mr. McIntosh's curves one notices that through virtually the whole of November the temperature is below the smoothed trend curve whereas it is above the trend curve almost continuously from mid-May to mid-July. Thus we must ask the physical question : are Mr. McIntosh's positive results due to the fact that certain periods of the order of a month in duration are relatively warm or cold compared with the two-term harmonic trend or are there periods of a few days with statistically significant departures ? This is an important question, for many forecasters have been led to suppose that certain calendar dates are particularly liable to cold spells and may weight the ideas in medium-range forecasting for a few days ahead.

Since the standard deviation of the monthly means from the chosen smooth curve is about 0·1°F while the standard deviation of the 3-day block means is only about 0·3°F it is not clear which is the more significant in the statistical treatments. I suggest that if we are to answer the definite question ' are there singularities of temperature dated within a few days ' we should compare the three-day block means with a graduated curve which eliminates the longer-period variations ; perhaps 20-day overlapping means would be better than a harmonic analysis. It is possible that by such a treatment the ' significance ' of the departures would be even less than that found by Mr. McIntosh.

Mr. D. J. Schove : Singularities, if real, presumably have physical causes. In April-May changes in continental interiors are so rapid (often exceeding 3°F/week) that large snow-covered areas become liberated about the same date each year. For the sake of argument let us suppose that the Ice Saints of May were for such reasons associated with a spurt in the differential fall of continental pressures (cf., Brier, G. W., *Trans. Amer. Geophys. Un.* **29**, Fig. 5, 1948), relative to those of the North Atlantic. The recent amelioration of the climate should make the date of such a cold spell earlier in the calendar since 1890 than formerly. From the author's Figs. 1 and 2 we notice that the main singularity in the months April-June occurred at the following approximate dates :

| | |
|---|---|
| *c* 1770-1840 | 10 May |
| *c* 1841-1897 | 5-10 May |
| 1901-1925 | 1-10 May |
| 1926-1950 | 30 April |

A secular trend in spring pressure has already been noted (*Weather*, p. 69, 1951) and international synoptic maps of snow cover combining Russian and American data might help in indicating whether the above considerations are likely to be of sufficient importance. The author has effectively disposed of singularities tied to the Scottish calendar and it would be asking for further mathematical sieve-work of an elaborate nature to dispose of the significance of a moving singularity. At the same time such an investigation should be done before we can claim that Buchan's periods are still real but ' arriving ten days early.'

Dr. J. M. Stagg : I feel that Mr. McIntosh's correlations and frequencies of departures are not as convincing as the other evidence he has so carefully produced. He has worked with exactly corresponding dates throughout each group of years; but though singularities may be linked with the calendar they are surely not linked quite so rigorously as that.

Mr. C. D. Ovey : I should like to draw attention to a possible cause – for what it may be worth – for the outstanding singularity for November as shown by Mr. McIntosh's curves. Every November, I believe, there is a ' storm ' of meteorites and the effect of meteoric dust in abundance during this period might be sufficient to reduce insolation and to affect temperature (according to J. G. Porter in *Comets and meteor streams*, London (Chapman and Hall), p. 70, 1952, the Leonids are at their maximum on 16-17 Nov.).

Dr. R. S. Scorer : The smooth seasonal variation with which the observations are compared is based on monthly values. By taking a shorter interval than a month it would be possible to obtain a curve from which the actual departures are indistinguishable from random. We should then have a meaningful curve indicating the condition at any time of year, the departure from which in any year could only be obtained by forecasting it a short time ahead. It is not clear how weather can be unseasonable in the mean except on account of some faulty definition of seasonable conditions. An objective which might satisfy Dr. Sutcliffe is to find a curve of seasonable variations of the kind just mentioned.

Dr. G. D. Robinson : I have read that the results of experiment or observation in general (not meteorological observations only), which are usually said to be satisfied by the Gaussian distribution, do in fact show an excess of large deviations. In my personal experience two or three sets of about a thousand observations of the vertical component of wind at 1/4 sec intervals had a distribution which was symmetrical, and Gaussian except for a small but highly significant number of large deviations. Can Mr. McIntosh give any further information on this ?

Mr. H. H. Lamb (*communicated*) : The author reaches a conclusion (p. 270) with which there will be broad general agreement, namely that ' the non-seasonal temperature fluctuations [at Edinburgh] are very largely of a random nature but it is highly improbable that they are entirely so.'

It will be agreed too that Buchan's (1867) belief that he had discovered a tendency for some of the apparently recurring temperature anomalies to shift their dates slightly in successive runs of years renders statistical verification of his work difficult, if not impossible, and opens the door wide to confusion with random fluctuations.

In other respects, however, it appears that McIntosh does less than justice to Buchan's pioneer work and the extent to which it has been checked by, and linked up with, more recent discoveries. One statement in particular (on p. 262) seems to need correction. Buchan originally used just *ten* years' records from 1857 to 1866 from five stations in different parts of Scotland to identify his warm and cold spells. To a very large extent therefore the rediscovery of these spells, first by Buchan himself in 40 additional years' records for Edinburgh alone and secondly by Mossman analysing 125 years' Edinburgh records from 1770 to 1894, does constitute an independent test.

In the present writer's opinion the most satisfactorily defined and verified singularities are (a) fourteen listed by Flohn and Hess (1949), as periods of 10-12 days of the year within which a given type of circulation pattern over Europe and the eastern Atlantic occurred on three or more successive days in 67 to 89 per cent of the years of the 66-year record studied; (b) a list by Baur (1948) of about twenty recurrent abnormalities in the pressure, temperature or rainfall curves for European – chiefly German – stations marked by frequency peaks exceeding the probable limit of chance. Baur uses as his criterion departures from the smoothed mean exceeding three times the standard deviation. The probability is only 0·27 per cent that such departures occur by chance.

Both Brooks (1946) and the present writer (1950) have found features of the frequency distribution of various weather types over the British Isles which show obvious correspondences with the known singularities for central Europe. Brooks further supplied a statistical verification of the non-random character of some of the related instrumental observations at Kew.

It is surely in terms of atmospheric-circulation patterns and processes, generated by (presumably) identifiable events in the seasonal progression over different parts of the northern hemisphere, that meteorological and climatological effects, recurrent and otherwise, are to be understood. It is, perhaps, to be expected that phenomena with a real tendency to recur about the same date of the year should be most marked, and most easily identified, over the continents, and especially over the eastern (leeward) parts of the continents, where the response to the primary seasonal changes of incoming and outgoing radiation is most immediate. It has been further suggested that certain events – notably the withdrawal about mid-October of the insolation from the (north) polar stratosphere (Flohn 1947) – set off pressure waves which show up in the alternating sequence of anticyclonic and cyclonic ' singularity ' phenomena in winter in northern and central Europe. Most of these interpretations may be regarded as unproven at

present, but it is interesting to note that they would lead us to expect recurrent (singularity) phenomena to be less marked, and less regular as regards date, at Edinburgh and more oceanic locations than at Kew and places nearer the heart of the continents or of the Arctic; on the other hand we should not expect Edinburgh to be entirely unaffected.

We may appropriately accord all the more honour to the pioneer British meteorologist of a century ago, who first spotted some of these effects in Scotland – albeit not without confusion with random and less noteworthy features. It is in the nature of singularity phenomena to show up less often in some runs of years than in others and the fact that few of Buchan's temperature singularities have clearly exceeded the limits of chance recurrence in the Edinburgh record for 1901-1950 need not unduly surprise us. I agree with McIntosh in finding more evidence for Buchan's six cold periods than for his three warm spells; one of the latter in any case seems only to mark the summit of the annual temperature curve in July. However, Buchan's warm spell in early December (3-10) comes on about the same date as a warm singularity (1-10) of proved significance in central Europe (Flohn and Hess 1949, see also Baur 1950).

## References


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|---|---|---|
| Baur, F. | 1948 | 'Zur Frage der Echtheit der Singularitäten, *Ann. Met.*, **1**, p. 372. |
| | 1950 | *Ann. Met.*, **3**, p. 74. |
| Brooks, C. E. P. | 1946 | *Weather*, **1**, Pp. 107 and 130. |
| Buchan, A. | 1867 | *J. Scot. Met. Soc.*, **2**, No. 13, p. 4. |
| Flohn, H. and Hess, P. | 1949 | *Met. Rund.*, Heidelberg, **2**, p. 258. |
| Flohn, H. | 1947 | *Experimentia*, Basel, **3**, No. 8, p. 1. |
| Lamb, H. H. | 1950 | *Quart. J. R. Met. Soc.*, **76**, p. 393. |


Dr. C. E. P. Brooks (*communicated*): The man who wrote 'I never saw a purple cow' was driven by too frequent quotation to lament 'I wish I had not wrote it.' That is rather my feeling over the paper by Mr. Mirrlees and myself on 'Irregularities in the annual variation of temperature of London,' which was intended mainly as an examination of the applicability of Buchan's periods to London, rather than as a general investigation of singularities. Unfortunately Mr. McIntosh does not quote my disclaimer in *Weather* for August 1946, in which a fuller study of all elements over western Europe, instead of merely London temperature, led me to the conclusion that these annual recurrences, or 'singularities,' are too frequent, and too regular (or rather, not sufficiently irregular), to be entirely unreal. In that article, pressure variations at Kew were treated in an exactly similar way to that which Mr. McIntosh employed with Edinburgh temperatures. The daily mean pressures 1881 to 1940 were plotted over a smooth curve computed by harmonic analysis and curves distant by 2 $\sigma$ on either side of the mean. The daily mean curve lay outside the 2 $\sigma$ curves on 25 days. When an allowance for persistence was made on the basis that only every third day was independent (compare Mr. McIntosh's 2·24) it was found that the expectancy by the theory of probability was only 17 days. The odds against 25 instead of 17 days of abnormal average pressure occurring by chance were calculated as some 400 to 1. While it must be remarked that only one or two of the periods of greatest pressure deviation at Kew coincide with those of greatest temperature deviation at Edinburgh, I agree with Mr. McIntosh's conclusions that while non-seasonal fluctuations are largely random, they are not entirely so.

Mr. D. H. McIntosh (*in reply, partly communicated*): In reply to Mr. Gold, I feel that the non-random element is so small as to hold out no hope of practical aid to the forecaster. Concerning the incidence of frosts, since daily mean and minimum temperature are highly correlated (mean was defined throughout as half maximum plus minimum) it seems unlikely that their recurrence characteristics would differ significantly.

Dr. Sutcliffe raises the important point of short versus long-period anomalies and I agree that the former are best shown, as he suggests, by first removing longer-period trends. The investigation was, however, concerned with identifying possible recurrences of either long or short duration. The recognition of a probable anomaly in mid-November holds good, I think, because the 'corrections' required for the presence of longer-period anomalies are demonstrably very small and it is quite uncertain where, during the year, any such correction should be applied. The long periods of relative cold and warmth mentioned by Dr. Sutcliffe are not to be regarded as real features but are sampling effects which are accentuated by being represented in 3-day block means: the set of random numbers with the same degree of coherence shows similar lengths of sequences of departures above or below zero.

Since the departures enumerated in Mr. Schove's interesting suggestion are, with one exception, of magnitude less than $2\sigma_n$, there is no reason to believe that they are other than random fluctuations. If real, they can be of only very small magnitude, and I cannot see that such small singularities of changing phase can be finally either proved or disproved.

In reply to Dr. Stagg, the coherence of the daily temperatures ensures that though, apparently, the unsmoothed departures on exactly corresponding dates only are correlated, the departures on 5 or 6 days on either side of exact correspondence are, to a certain degree, in fact correlated. The frequencies of departures show similar coherence.

Regarding the point raised by Dr. Scorer, I believe that to define the seasonal curve by, e.g., running means over an interval appreciably shorter than a month would be to give undue weight to fluctuations which the analysis has shown to correspond to random effects. I do not think that we can define a ' meaningful ' seasonal curve more precisely than by the two-term harmonic approximation from monthly values, with small amendments (precise magnitude unknown) in mid-November and at a few other times which cannot yet be specified.

I am indebted to Dr. Robinson for pointing out that series of actual observations often show an excess of large deviations as compared with the Gaussian distribution and find that this is a feature which has been discussed, e.g., by Jeffreys (*Proc. Camb. Phil. Soc.*, **31**, p. 203, 1935). If this is a feature of the temperature departures, it may obviously explain the excess of large deviations found in the period means. I have counted the numbers of large departures in 10 *individual* years (1926-35) and find that the mean annual numbers of departures $\geqslant 2{\cdot}0\ \sigma$, $\geqslant 2{\cdot}5\ \sigma$, $\geqslant 3{\cdot}0\ \sigma$ are respectively 15·4, 4·0, 1·1. These means have respective standard errors 1·5, 1·0, 0·4 and compare with Gaussian expectancies of 16·8, 4·4, 0·9. The feature mentioned by Dr. Robinson is thus not shown by the temperature departures, and the conclusion drawn in the paper that the excess of large departures in the period *means* arises from a small non-random tendency for similar departures on corresponding dates in different years is therefore not invalidated.

I cannot agree with Mr. Lamb that I have not done justice to Buchan's work in this field but agree that more examples might have been given of the way in which it has been to some extent supported by later work. Buchan used such methods of analysis as were then available, but made approximations which the very small size – as we may now see it to be – of the non-random element renders unjustified. Thus, for example, since he compared mean daily temperatures for a 10-year period with the seasonal values of an independent period of 40 years, some of the large departures on which the spells were based were almost certainly the result, in part, of a bad ' fit ' between the temperatures of these two periods. Nor do comparisons with the departures in the check periods mentioned by Mr. Lamb carry any conviction of the reality of the nominated spells. I agree that European singularities appear to be more firmly established than those in the British Isles. However, the allotment of significance to all days with a departure in excess of $3\ \sigma$ must lead to an appreciable overestimation of the non-random element : the quoted probability of 0·27 per cent is not a measure of the dependability of all the singularities selected on this basis, but is merely the probability of a single departure, *selected at random*, exceeding $3\ \sigma$.

The non-random element in Kew pressure referred to by Dr. Brooks is likely to bear some relation to that found in Edinburgh temperature, though the relationship is probably complex. It is certainly of interest that the size of these two elements is about the same.

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551.583.14

# Climatic fluctuation in the Greenland and Norwegian seas

By P. R. BROWN

(Read 20 May 1953. See *Q.J.*, **79**, p. 272)

Dr. J. Glasspoole : In Table 2 of Mr. Brown's paper, the squares which cover most nearly the Shetlands are 217A and 252A, but the changes of air temperature given for these squares show marked differences. It would be most useful if Mr. Brown could give the decadal changes of temperature, using decades ending in 0, for the Shetland area, so that a comparison can be made with temperature changes at land stations there.

Cdr. C. E. N. Frankcom : Mr. Brown's paper provides a good and practical example of the inescapable relationship between meteorology and oceanography and stresses the important part that sea temperature and ocean currents may play in meteorological developments. I presume that the variability from year to year of the sea and air temperature off the Greenland coast is very much influenced by the amount of ice which is brought down that coast by the East Greenland Current. Is it not possible that a study of ice conditions from year to year on the Banks of Newfoundland, which are known to vary very considerably, might provide some useful information about the effect of ocean currents upon climatic conditions in Europe in the following season ? Can Mr. Brown say if an investigation of air and sea temperature in the vicinity of the North Atlantic Drift (in mid-Atlantic) on an annual basis might correlate in a similar manner with weather conditions, say the following year on the Norwegian Coast ?

Mr. D. J. Schove : Mr. Brown's very useful data for the ocean seem to support and amplify inferences about the increase in the circulation and the backing of the prevailing wind (e.g., Schove, Q.J., **76**, p. 147, 1950). The great increase in south-westerliness in the North Atlantic, marked for example by low pressures in North Norway, in the twenties is linked not only with the warming of the Arctic generally but with the interesting oscillation mentioned between temperatures south of Iceland and those west of Norway. This is well illustrated by the maps of Sandström (*Ark. Ma. Astr. Fys.*, **28**A, No. 1, Pl. I, 1943), which show for individual Januarys that high-index situations (in high latitudes) favour warm air near Norway and cold near Greenland. Sea temperatures would seem to be affected in the same way. From the temperature data given, the increased circulation would seem to have affected the 1930's especially (cf., R. Scherhag, *Ber. Deutschen Wetterdienstes, U.S. Zone*, No. 12, p. 40, 1950) and the backing of the prevailing wind to have continued into the 1940's. Perhaps Mr. Brown can tell us whether pressure data prepared in the same way will soon be available to examine these changes barometrically. I would also like to ask whether any significance (e.g., in relation to observed trends in visibility or thunder frequency) can be attached to maps of the differences between air and sea temperatures. I presume that Smed's data are largely independent, and, as they include coastal data (e.g., Iceland and the Faroes), I hope it will prove possible to use combined results for maps of air and sea anomalies in which each year of the decade has equal weight. In any case the technique here initiated is most promising and I look forward very much to future results.

Mr. A. J. Whiten : I presume that the sea-water temperatures mentioned in Mr. Brown's paper are surface temperatures. Deeper temperatures would no doubt be markedly influenced by currents, but might show significant effects.

Mr. E. Gold : I should like to ask Mr. Brown if he compared the results for squares 217A and 217B with the corresponding records from Lerwick and Thorshavn. May I also say that, although the accidental errors in ships' observations balance out in the long run, this does not apply to the error due to the effect of the ship on the temperature, which is generally to make the reading too high. The effect may not be very large in the latitudes dealt with in Mr. Brown's paper but it can be substantial in lower latitudes.

Mr. P. R. Brown (*in reply, partly communicated*) : In reply to Cdr. Frankcom, many investigations have been carried out into the relationship between variations of sea temperature in the region of the North Atlantic Drift and those of air temperature in the region of north-west Europe. One of the most recent investigations Bergsten (*Geogr. Ann.*, Stockholm, **18**, p. 298, 1936), correlated the sea-surface temperature in the rectangle 59°-61°N, 10°-30°W in the season April to October with the air temperature at various points in north-west Europe the following winter (December to March). Drawing isopleths of the correlation coefficients he found the greatest coefficients, just over 0·40, in the area west of Norway. The coefficients, however, were not large enough for practical forecasting.

In reply to Mr. Schove, I agree that the intensity of the atmospheric circulation seems to be linked with the air and sea-surface temperature variations and the oscillations between the fluctuations in the areas south of Iceland and west of Norway. This oscillation, I think, may be general in the high latitudes of any ocean which is bounded on both sides by land masses but which is open to high latitudes. I have found a similar tendency for an oscillation during the southern summer (December to February) in the latitudes 50°-60°S between longitudes 80°-130°W and 150°W-140°E. Here presumably an increased cyclonic circulation, caused by an increased number or the greater intensity of depressions travelling south-eastwards in the Pacific sector of the Southern Ocean, will draw more warm air and surface ocean water southwards in the east of the ocean and more cold air and water northwards in the west of the ocean. With regard to Mr. Schove's question about pressure data, it would not be possible to obtain many reliable decadal means of pressure at higher latitudes than 60° owing to the comparatively small number

of observations available, but I am considering the possibility of preparing decadal mean pressure charts for the Atlantic, which would cover most of the area between 60°N and 60°S. With regard to the differences between mean air and mean sea temperature, I think that these can be regarded as reliable; since most ship observations contain reports of both, the differences of the mean will be the mean of the differences of the individual observations. Observations are not sufficient to obtain temperature anomalies for every individual year in many areas in high latitudes, but I hope to prepare anomalies for individual years in some temperate latitude areas.

In reply to Mr. Whiten, all the water temperatures were those of the sea-surface layer. While the atmospheric circulation is one major factor in the transport of energy, the water circulation, comprising ocean currents at all levels, must be another major factor in the transport of heat and hence in the creation of the temperature variation.

In reply to Mr. Gold, I compared the sea temperature anomalies at Myggenaes light (62° 06′N, 7° 40′W) in the Faroes, as given by Smed (*Cons. Int. Expl. Mer. Ann. Biol.*, Copenhagen, **8**, p. 20, 1951), with those for square 217B, the centre of which is near Myggenaes. The anomalies given by Smed are based on a mean for the years 1914-1950 :

| | 1914-9 | 1920-9 | 1930-9 | 1940-9 |
|---|---|---|---|---|
| Myggenaes | − 0·4°C | − 0·2 | + 0·1 | + 0·3 |
| | 1910-9 | 1920-9 | 1930-9 | 1940-9 |
| Square 217M | − 0·4 | 0·0 | + 0·4 | + 0·3 |

I agree with Mr. Gold that, while the accidental errors in ships' temperature observations tend to cancel out, the heating effect, while probably small in high latitudes, is likely to be appreciable in low latitudes. However, while the absolute values of the means in low latitudes are probably too high, I doubt whether the errors in the means so caused are likely to vary greatly from decade to decade and so falsify the character of the variations of decadal temperatures for a particular area.

In reply to Dr. Glasspoole and Mr. Gold concerning the comparison with the Shetlands' data, the latter cannot be expected to agree very closely with means for square 217A, since the Shetlands are on the edge of this area. I have therefore worked out the air temperature means for decades ending in 0 for the sea area just around the Shetlands. The area used was 58°-63°N, 00-03°W. The results obtained were as follows, being given in °F for easy comparison with any land data :

| 1901-10 | 1911-20 | 1921-30 | 1931-40 | 1941-50 |
|---|---|---|---|---|
| 47·2 | 47·5 | 47·4 | 48·3 | 48·3 |

Assuming that the differences between the means for the decades ending in 9 and 0 are small, the variation between 1901 and 1940 shows a similar character to that for square 252A, but a smaller overall rise of temperature. Smed has shown that the rise in temperature during the last few decades in the higher latitudes of the north Atlantic decreased generally with decreasing latitude. The decadal annual mean temperature for the Shetlands area for 1941-50 was the same as that for 1931-40, whereas in the squares along the Norwegian coasts those for the 40's were considerably lower than those for the 30's. However, it will be noticed from Table 2 in the paper under discussion that this fall in temperature decreased in amount southwards from the area of northern Norway and could be expected to be small or non-existent in the Shetlands area. Dr. L. H. N. Cooper, Marine Biological Association, in a letter to me on this paper, suggests that the factors affecting the temperature variation in these squares off the Norwegian coast are likely to include, in addition to the circulation of the atmosphere and wind-driven ocean currents, the effect of the compensation water drawn in from the Atlantic to compensate for cold water cascaded from the Siberian Shelf : we might expect this effect to decrease southwards from the region of northern Norway.

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## The mean temperature of central England, 1698-1952

By G. MANLEY

(Read 20 May 1953. See *Q.J.*, **79**, p. 242)

Dr. J. Glasspoole : I am not convinced that the best procedure has been used in this paper to define the temperature trends of earlier years. I have advocated, firstly, a statistical study of temperature variations during the past 50 or 100 years, for which many data are

available, then the critical examination of the data, by using say the standard deviation, to test records for homogeneity. This would lead to the enumeration of the causes of the breaks in the homogeneity and the definition of the amounts involved in any changes of screens, exposures or method of recording. Using such methods, I think the earlier records could be used with greater understanding and more reliable results obtained. This was the method adopted for rainfall, as set out in *British Rainfall*, 1923 and 1925.

With the present title ' The mean temperature of central England, 1698-1952 ' it is surprising to find that the paper gives serial values of monthly and annual mean temperature, over 3,000 in all, and I should like to enumerate some of the variations hidden in any one of these mean values :

1. Each part of central England has its own distinct variation with time. The changes of temperature, year by year, or decade by decade, over the country can be plotted and mapped. These changes ought not to be neglected, when they can be defined for at least 100 years, and these variations ought to be studied, I think, to throw light on the probable variations in earlier decades. Stations used in this paper vary mainly between Halifax and Plymouth, omitting the further extensions to Edinburgh and Utrecht. Now the correlation coefficient of the annual values, 1921-1950, at York and Plymouth is + 0·83. This implies that about two-thirds of the annual fluctuations can be regarded as having a common cause and one-third are distinct, and it is these distinct variations which ought not to be overlooked.

2. Then the variability of the annual values of mean temperatures is not the same in all parts of the country. The mean deviation, 1921-1950, varies from 0·75°F in parts of the east between Edinburgh and Nottingham and Spurn Head to 0·55°F in the west at Holyhead and Plymouth. Clearly the mean results, as given in this paper, are dependent upon the stations selected, their number and distribution.

3. The seasonal variability is also not the same in all localities. The monthly mean deviations, 1921-1950, for York and Plymouth are set out below to illustrate this point.

Values of mean deviation, °F, 1921-1950

| | J | F | M | A | M | J | J | A | S | O | N | D | Year |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Plymouth | 2·3 | 2·5 | 1·6 | 1·5 | 1·3 | 1·5 | 1·6 | 1·4 | 1·7 | 1·3 | 1·6 | 2·2 | 0·55 |
| York | 2·8 | 2·7 | 2·3 | 1·8 | 1·4 | 1·5 | 1·5 | 1·4 | 1·6 | 1·4 | 1·6 | 2·3 | 0·75 |

The greater variability occurs in the east and in the months Dec. to Apr., owing presumably to the chance of cold air from the east and north-east. As Mr. McIntosh puts it in his paper on ' Annual recurrences in Edinburgh temperature,' p. 271, ' any real anomalies are mainly negative in character.'

4. An important variation of temperature is due to altitude, but this paper curiously hardly refers to altitude. In the *Monthly Weather Report* of the Meteorological Office the procedure of taking means of station values, as in this paper, was abandoned in 1928, because the homogeneity of the series of general values was broken with each change of station, owing to the inevitable height changes and the complexities and errors introduced by applying factors. Since then the procedure has been adopted of taking the means of departures from station averages, five in each district, with nine districts covering Great Britain. These mean departures can be applied to general averages for England and Wales, and for Scotland, based on maps of the mean temperature at sea-level, to give a more uniform series of values month by month or year by year. Prof. Manley makes no reference to a number of earlier papers dealing with temperature trends, including that published in the *Journal* for January 1942, or to these main factors which are involved of altitude, local trends, annual variability or seasonal variability.

Maps of mean temperature at sea-level for the months and the year are available in the *Monthly Weather Report* for over 50 years, from which Prof. Manley could have determined better values for the temperature of central England than those given in this paper. I have found it impossible, however, to make any detailed comparison because the area of central England used in this paper is not defined as regards height or extent. The general values for England and Wales, and Scotland, which are given in the *Monthly Weather Report* are enabling a set of values to be built up month by month. On the other hand, the values in this paper cannot readily be continued as they are likely to break down as station changes occur. We are

already preparing for a possible break in the record at Oxford, which is used extensively in this paper, by establishing another station for which overlapping records are available for about a year, and moreover the site of the present record became more sheltered in 1935.

Prof. Manley has used his values to illustrate trends of temperature by running 10-year means for January and July (*Q.J.*, p. 193, 1953). On the scale of his diagram the trends do in fact show close qualitative accord with those for the British Isles since 1880, published in the *Q.J.*, p. 53, 1942, and it would, I think, have been more useful to have continued to earlier times one of the series included in this paper, than to have introduced a new indefinite area of central England.

I hope Prof. Manley will make available in the Library of the Society, and to individual workers, the values he has worked out for each station, so that they may be available for investigation. It is clear that an enormous amount of work has gone into the preparation of these station values, and I should like to see Prof. Manley's work used to the fullest extent.

Mr. D. J. Schove : I think that, with regard to Professor Manley's tabulation, Dr. Glasspoole would agree that anomalies in relation to 1901-1930 means would in practice be much the same whatever height a.s.l. or region of England was selected.

I am particularly interested to hear of the newly-discovered diaries which Professor Manley has been able to utilize in his table. Perhaps these English diaries can throw light on the validity of the corresponding Dutch series of the early eighteenth century. The late Dr. Labrijn left his Dutch series in two parts with a break between 1734 and 1735, and stated that his earlier data were less reliable. The discrepancy between the upward trend of the thermometric data and the downward trend of Easton's documentary evidence (from 1701-1710 to 1731-1740) led to a discussion (*Q.J.*, **73**, pp. 195-197, 1947) in which Labrijn stated that his values for the 1730's were not ' too high by 1°C ' and Professor Manley has already (*Q.J.*, **74**, pp. 119-122, 1948) discussed the relevance of the Plymouth series in this connection. I should very much like to know if the new series do solve this problem.

Mr. E. Gold : I venture to suggest that the mean temperatures for the whole year should be given in a supplementary table as it has not been possible to include them in Table 4 – I mean for each year from 1699 to 1952. Possibly also it would be worth while giving the range for each year from the coldest month of the winter to the warmest month of the following summer. In 1947 this was 37°F, nearly double the range for the following year, 20°F.

Dr. C. E. P. Brooks (*communicated*) : Professor Gordon Manley's painstaking researches into the temperature of Britain are gradually extending our statistical knowledge of climatic variations in this island backwards into what have hitherto been years of conjecture. Anyone who has had to combine two or more series of observations into a single series will know what a large amount of work is involved, and how often it is necessary to hold a balance between arithmetic and common sense. In my reduction of the early London observations for Sir David Brunt, for example, I recognize now that in respect of the end of the 18th century I may have accepted the arithmetic too uncritically, with the result that the reduced values may (or may not) be too high. With a much greater quantity of material to work with, Prof. Manley has tried to avoid this type of error, partly by a series of cross checks and partly, it appears, by supplementing arithmetic with common sense. The resulting figures show a remarkable degree of uniformity in British climate, as opposed to variations from one year to another. The variation of climate over two and a half centuries seems to be considerably less than would be inferred from non-instrumental records and popular beliefs such as the ' old-fashioned winter.' Presumably we should learn from this that we must be cautious in assessing pre-instrumental records of warmth and cold, as C. E. Britton has taught us to be in dealing with droughts and heavy rains. But we must not fall again into the error of some of the older climatologists, whose ' common sense ' told them that there had been no changes of climate at all during the historical period, and so led them to regard all evidence to the contrary as untrustworthy. Table 6 shows nothing in previous decades to equal the warming up in the 20th century. Mr. Callendar would attribute this to the increasing amount of $CO_2$ in the atmosphere, but my recollection is that some other long series suggest the contrary. Coming back to the Sion House figures for the end of the 18th century, the explanation that the high temperatures were apparent only, owing to the breaking of the thread of the thermometer, may be correct, but one would have thought that a reasonably careful observer would have noticed such a rather obvious source of error. Miss L. F. Lewis remarked (*Meteorological Magazine*, p. 138, 1947) that the extraordinarily high temperatures recorded in London coincided with a period of remarkably steady southerly winds in London (but not in Edinburgh). Is it possible that in rejecting these temperature readings Prof. Manley has given too much weight to ' common sense ' ?

There is one other point : could we please have a table of seasonal and annual means for each year from 1698 to 1952, to help in playing with the figures ?

Mr. E. L. Hawke (*communicated*): Prof. Manley deserves our congratulations on his heroic attempt to construct a general picture of the mean temperature in central England month by month through 2½ centuries. The vast amount of time and labour that he has bestowed during recent years on the analysis and checking of ancient records has certainly been well spent. I have long shared Prof. Manley's doubts as to the validity of the ' London ' figures given in Alexander Buchan's tables for the closing decades of the 18th century. In the discussion of a paper on ' Climatic cycles ' read by Sir David Brunt before the Royal Geographical Society on 18 January 1937 I cited Gilbert White's Selborne observations as evidence against the reality of the sustained and excessive summer heat which would appear from Buchan's estimates to have occurred between 1781 and 1785. I stated my belief that there were radical errors in some of the temperature data which Buchan used when evaluating the means for this period and suggested that he would have been wise to reject them.

It is satisfactory that Prof. Manley has been able to make so much of readings from Hauksbee thermometers. Hard things have been said about these. In Vol. 20 of his *Monthly Meteorological Magazine*, p. 73, G. J. Symons expressed the opinion that ' M. Wargentin was quite right when he wrote (*Mém. Acad. Suède*, xi, p. 178) respecting Hauksbee's thermometers, " Ces thermomètres ne valent absolument rien".' Symons had previously criticized Huxham's observations at Plymouth with one of these instruments on the grounds that they gave an annual mean temperature equivalent to 59°F (' far too high ') and a value representing about 23°F on what Huxham described as ' the coldest day in the memory of man'; this, Symons maintained, was nothing like low enough to fit in with Huxham's description of the effects of the cold. It would seem that Symons's mode of effecting a Hauksbee-Fahrenheit conversion was not quite the same as Prof. Manley's.

As to the relationship between ' cool-room ' and outdoor mean temperatures, it might be useful to have some modern comparisons. There must be climatological stations at which one or more regular daily readings of the thermometer attached to a standard barometer exposed in a fireless room with a northward aspect have been recorded over a series of years. To extract monthly means from these readings and ascertain how they compare with the means given by thermometers in the Stevenson screen would be no heavy task and would provide some sort of check on the entries in Prof. Manley's table which are derived from indoor observations ' on Jurin's system.'

Prof. G. Manley (*in reply, partly communicated*) : Dr. Glasspoole has long been recognized as an authority on the fluctuations of British rainfall. With regard to fluctuations of mean temperature however I have not known his work to go beyond a reduction of the seventy-odd years of official records in the paper in which he collaborated with Mr. Hogg and to which he refers (*Q.J.*, 1942). He is surely aware that Buchan's 1893 papers and tables were provided with titles similar to those which I have used; my paper is not a mere provision of serial values and I think Dr. Glasspoole is the only user of that term; if, in respect of temperature, he had paid more attention to the paper under discussion he would have seen that I had indicated the region for which the figures given are likely to be representative; also, that I had not provided serial values of annual mean temperature. I shall accordingly take leave to reply in more detail, later in this discussion, to the statement which he has evidently prepared.

I am grateful for Mr. Schove's recognition that the anomalies are more important than the precise altitude above sea-level. With regard to the break in Labrijn's table (1734-5) I think his values through the 1730's were as reasonable as one might expect, on the assumption that my own values are reasonable. From 1727-1739 the central England values derive largely from Southwick which I regard as obviously likely to possess greater weight than the values derived from distant Plymouth. We do not know whether there were changes during that period but certainly no mention is made of any lack of homogeneity. For the years 1727-39 the annual mean for Utrecht differs from ' central England ' in °F by − 0·8, − 0·7, − 0·1, − 0·1, + 0·1, 0·0, + 0·3, − 0·1, + 0·6, − 0·3, − 1·1, − 0·3; the mean of these is − 0·2; the present day difference being (1871-1950), − 0·3°F. All things considered, I think we can reasonably accept continuity in the Utrecht values; also, Utrecht is almost certainly *not* 1°C, or 1·8°F, too warm during the period 1730-1739. If the standardization of central England were strictly reliable throughout, those Utrecht annual values might be of the order of 0·2 or 0·3 too warm, but this is far from the 1·8°F suggested by Mr. Callendar. The possibility remains that ' central England ' for that decade is by the like amount too cool; observations of snow-frequency however do not support this. Nevertheless I am a little doubtful whether we can yet press even the

annual means to such close limits as 0·2°F and it is only fair to say so, in the present state of our knowledge. The accordance of my figures with the known behaviour of Icelandic glaciers is nevertheless reassuring.

I take with pleasure the opportunity of thanking Mr. Gold, Dr. Brooks, and Mr. Hawke for their contributions. In reply to Dr. Brooks I much appreciate what he says about holding a balance between arithmetic and common sense. Dependent as one must be before 1752 on highly imperfect observations at stations far apart, reduced through a fourfold series of assumptions, one is frequently brought to the stage of weighing the chances between three or four values of a monthly mean; and an element of judgment must still remain in respect of the quality of snow-frequency observations used as a check, as Mr. Ashmore has recently demonstrated. The variation of our temperature climate is indeed small, but as regards winter a drop of three degrees in January means all the difference between Kew and Buxton in regard to impressions of snowfall; likewise, a cloudy and sunless summer may be but a little cooler, but makes a keen impression on our senses. I think that before the days of thermometry the opinions of a good many of our writers were thus affected. The high Sion House temperatures cease rather suddenly and perhaps were due to radiation effects, but as the 8 a.m. reading appears also to have been affected I was inclined to think a broken thread might be the cause. I found recently (R.S. Library) a completely independent check on my proposed July means for 1783 and 1784 in readings kept at Minehead; a shaded mercury thermometer, details lacking, was read at 06 hr, 12 hr and 21 hr. For July 1783 the mean was 66·8° and for July 1784, 59·7°; my table gives 65·8° and 59·3° for the more northern west Midland region. As the highest noon reading was 80° the instrument was well protected. I am glad to be able to provide in the Tables 1, 2 and 3 given below, the further values of seasonal and annual means for Dr. Brooks ' to play with '; I hope, too, that they will prove useful to Mr. Gold. I appreciate his regard for the significance of the range between the coldest and warmest month; I have ventured to provide a diagram of running means of the January-July range for comparison with those published elsewhere (Fig. 1).

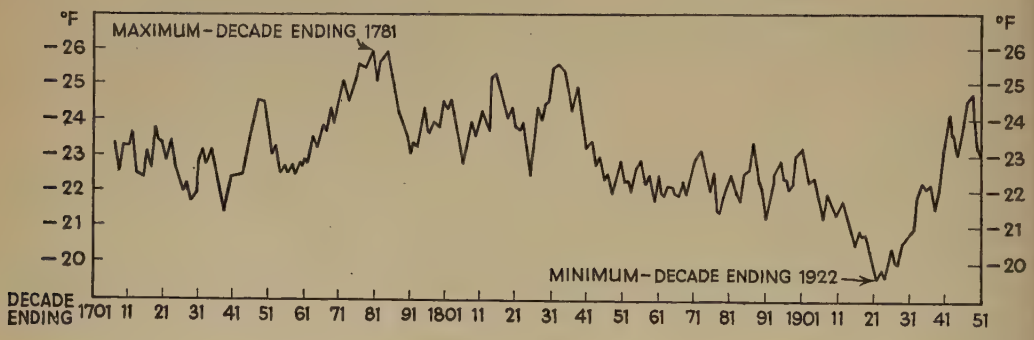


Figure 1. Decadal running means of the range of mean temperature, January-July

I wish to thank Miss G. Berry, one of my students at Bedford College, for assistance in the compilation of these tables.

For the sake of those readers who, on receiving the *Monthly Weather Report*, wish to obtain figures for comparison with those of the tables below I would explain that approximate values for each month can most easily be obtained by averaging the anomalies, or departures from the mean 1921-1950, for characteristic west Midland stations such as Shrewsbury, Leamington, Belper, Welshpool. Of the above, Shrewsbury is perhaps the best single representative. The monthly means (°F) for 1921-1950 for ' central England,' to which the above anomalies may reasonably be applied are given in Table 3.

Mr. Hawke will also no doubt be interested in the Minehead data below; the mean for July 1783 is also supported, approximately, by shade readings at Mongewell and by indoor readings at Fyfield (*Gentleman's Magazine*). Wargentin's remarks (about 1742) may have arisen not only from loyalty to Celsius who was then at work in Uppsala, but also from the fact that he had, we may presume, direct knowledge of the defects of the earlier readings as one of Haukbee's instruments appears to have been in use at Uppsala in 1722 (MS. observations in Royal Society's Library). My estimate of Huxham's lowest reported temperature, the ' coldest day in the memory of man.' lies between 22°F and 23°F. This was indoors and there was a very

TABLE 1. REPRESENTATIVE VALUES OF THE SEASONAL AND ANNUAL MEAN TEMPERATURE (°F) FOR CENTRAL ENGLAND, 1698-1952

('Winter' includes December of the previous year)

| | Win. | Spr. | Sum. | Aut. | Year | | Win. | Spr. | Sum. | Aut. | Year |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1698 | (35 +) | 44·0 | 59·0 | 47·7 | 46·8 | 1761 | 42·4 | 48·9 | 59·9 | 49·9 | 50·0 |
| 1699 | 38·8 | 43·7 | 59·8 | 49·3 | 47·8 | 1762 | 39·9 | 48·0 | 62·0 | 47·7 | 49·3 |
| 1700 | 38·8 | 46·5 | 57·8 | 48·3 | 47·9 | 1763 | 36·6 | 46·7 | 59·1 | 48·3 | 48·1 |
| | | | | | | 1764 | 40·2 | 46·0 | 59·1 | 47·5 | 47·7 |
| 1701 | 37·8 | 42·7 | 60·8 | 48·8 | 47·5 | 1765 | 36·8 | 46·3 | 58·8 | 47·8 | 47·3 |
| 1702 | 41·0 | 45·2 | 58·7 | 49·2 | 48·4 | 1766 | 34·4 | 45·2 | 59·6 | 49·9 | 47·5 |
| 1703 | 37·7 | 46·2 | 58·2 | 47·8 | 47·7 | 1767 | 37·3 | 45·2 | 58·0 | 50·1 | 47·7 |
| 1704 | 38·3 | 46·8 | 60·2 | 48·0 | 48·2 | 1768 | 37·4 | 47·0 | 59·3 | 47·9 | 48·1 |
| 1705 | 38·2 | 44·8 | 58·7 | 46·7 | 47·2 | 1769 | 37·9 | 46·4 | 58·7 | 48·0 | 47·8 |
| 1706 | 39·0 | 48·0 | 60·3 | 49·0 | 49·1 | 1770 | 39·8 | 42·8 | 58·5 | 48·8 | 47·3 |
| 1707 | 38·7 | 46·7 | 61·7 | 49·3 | 48·8 | | | | | | |
| 1708 | 40·0 | 48·0 | 59·7 | 50·3 | 49·4 | 1771 | 36·7 | 44·5 | 58·6 | 48·6 | 47·4 |
| 1709 | 33·7 | 46·3 | 59·3 | 51·3 | 48·0 | 1772 | 37·2 | 44·5 | 61·5 | 51·1 | 48·5 |
| 1710 | 38·7 | 46·7 | 58·7 | 50·3 | 49·0 | 1773 | 38·8 | 47·0 | 60·6 | 48·6 | 48·6 |
| | | | | | | 1774 | 37·2 | 47·5 | 60·1 | 48·6 | 48·3 |
| 1711 | 41·0 | 47·7 | 59·3 | 49·7 | 48·7 | 1775 | 40·5 | 49·0 | 61·5 | 49·1 | 50·1 |
| 1712 | 38·3 | 46·3 | 59·3 | 49·0 | 48·6 | 1776 | 36·0 | 48·0 | 59·3 | 49·6 | 48·2 |
| 1713 | 40·3 | 43·0 | 57·3 | 48·7 | 47·1 | 1777 | 37·2 | 47·4 | 58·9 | 51·0 | 48·3 |
| 1714 | 40·0 | 45·3 | 60·0 | 49·7 | 48·7 | 1778 | 36·6 | 46·0 | 61·7 | 47·8 | 48·7 |
| 1715 | 39·7 | 48·3 | 58·3 | 50·7 | 48·8 | 1779 | 42·2 | 49·5 | 61·9 | 51·1 | 50·7 |
| 1716 | 33·0 | 46·3 | 58·7 | 48·3 | 46·8 | 1780 | 34·5 | 48·3 | 61·1 | 49·5 | 48·4 |
| 1717 | 38·0 | 45·0 | 58·3 | 49·0 | 47·9 | | | | | | |
| 1718 | 37·3 | 46·7 | 61·3 | 50·0 | 48·7 | 1781 | 38·0 | 48·7 | 62·5 | 50·8 | 50·4 |
| 1719 | 39·3 | 46·0 | 61·7 | 49·3 | 48·9 | 1782 | 39·5 | 43·0 | 58·8 | 45·9 | 46·4 |
| 1720 | 39·0 | 45·3 | 57·7 | 48·7 | 48·0 | 1783 | 37·7 | 46·3 | 61·6 | 49·3 | 48·7 |
| | | | | | | 1784 | 34·1 | 45·1 | 57·7 | 48·8 | 46·1 |
| 1721 | 39·3 | 44·3 | 58·3 | 49·3 | 47·7 | 1785 | 34·5 | 45·2 | 59·7 | 48·8 | 47·4 |
| 1722 | 40·0 | 46·7 | 58·3 | 50·7 | 48·7 | 1786 | 37·3 | 44·8 | 59·7 | 45·5 | 46·8 |
| 1723 | 37·7 | 48·8 | 59·5 | 51·2 | 49·6 | 1787 | 39·4 | 47·5 | 59·1 | 48·3 | 48·7 |
| 1724 | 41·3 | 46·0 | 59·9 | 49·0 | 48·7 | 1788 | 38·9 | 48·1 | 60·2 | 49·6 | 48·6 |
| 1725 | 38·7 | 46·3 | 55·6 | 49·5 | 47·6 | 1789 | 35·7 | 45·2 | 59·6 | 47·8 | 48·0 |
| 1726 | 37·8 | 47·6 | 60·8 | 50·6 | 48·8 | 1790 | 42·2 | 46·7 | 59·1 | 49·1 | 49·0 |
| 1727 | 38·9 | 48·7 | 61·3 | 50·0 | 49·9 | | | | | | |
| 1728 | 37·9 | 48·7 | 61·6 | 49·5 | 49·1 | 1791 | 40·0 | 48·0 | 59·5 | 49·2 | 48·7 |
| 1729 | 35·0 | 44·1 | 60·6 | 52·8 | 48·7 | 1792 | 36·8 | 47·6 | 59·3 | 48·6 | 48·6 |
| 1730 | 40·2 | 48·4 | 59·4 | 53·2 | 50·1 | 1793 | 39·0 | 44·8 | 59·6 | 49·4 | 48·4 |
| | | | | | | 1794 | 40·6 | 49·1 | 61·6 | 48·9 | 49·8 |
| 1731 | 36·5 | 46·9 | 61·1 | 53·2 | 49·7 | 1795 | 32·9 | 45·5 | 58·9 | 51·3 | 47·6 |
| 1732 | 40·3 | 47·9 | 60·3 | 51·0 | 49·5 | 1796 | 43·2 | 46·8 | 58·7 | 48·3 | 48·2 |
| 1733 | 40·8 | 48·3 | 61·8 | 49·0 | 50·8 | 1797 | 37·0 | 45·8 | 60·0 | 47·3 | 48·2 |
| 1734 | 43·0 | 49·1 | 59·8 | 48·7 | 49·6 | 1798 | 39·5 | 49·1 | 61·7 | 49·0 | 49·3 |
| 1735 | 39·5 | 47·4 | 58·6 | 50·5 | 49·2 | 1799 | 35·6 | 43·0 | 58·2 | 48·1 | 46·2 |
| 1736 | 44·3 | 47·7 | 61·9 | 51·1 | 50·5 | 1800 | 35·8 | 47·4 | 61·0 | 49·2 | 48·7 |
| 1737 | 42·1 | 48·4 | 60·3 | 49·5 | 49·9 | | | | | | |
| 1738 | 40·5 | 48·1 | 59·9 | 49·4 | 49·7 | 1801 | 39·7 | 48·2 | 60·8 | 49·5 | 49·6 |
| 1739 | 42·1 | 46·4 | 59·5 | 47·8 | 48·5 | 1802 | 36·1 | 47·0 | 58·6 | 49·4 | 48·1 |
| 1740 | 31·3 | 43·3 | 57·7 | 45·6 | 44·3 | 1803 | 37·3 | 47·5 | 60·4 | 47·5 | 48·3 |
| | | | | | | 1804 | 39·9 | 46·9 | 60·5 | 50·8 | 49·2 |
| 1741 | 37·0 | 44·3 | 60·4 | 52·1 | 48·5 | 1805 | 37·0 | 46·8 | 59·4 | 48·5 | 48·1 |
| 1742 | 37·7 | 44·7 | 60·0 | 47·5 | 47·1 | 1806 | 39·3 | 46·3 | 59·9 | 51·1 | 49·6 |
| 1743 | 38·1 | 46·4 | 60·4 | 51·4 | 49·7 | 1807 | 40·0 | 45·5 | 60·9 | 46·9 | 47·6 |
| 1744 | 37·5 | 45·4 | 59·8 | 49·5 | 47·8 | 1808 | 36·4 | 45·7 | 61·9 | 47·5 | 47·9 |
| 1745 | 37·8 | 46·0 | 58·0 | 50·2 | 47·9 | 1809 | 38·0 | 46·6 | 58·2 | 48·5 | 48·1 |
| 1746 | 35·9 | 45·7 | 59·6 | 47·2 | 47·5 | 1810 | 37·9 | 45·4 | 58·7 | 49·5 | 47·8 |
| 1747 | 40·7 | 45·7 | 62·0 | 50·5 | 49·7 | | | | | | |
| 1748 | 37·8 | 43·1 | 59·7 | 50·3 | 47·8 | 1811 | 37·6 | 49·2 | 58·8 | 52·2 | 49·4 |
| 1749 | 41·0 | 46·7 | 58·8 | 50·3 | 49·0 | 1812 | 38·6 | 44·0 | 56·9 | 48·4 | 46·8 |
| 1750 | 41·2 | 48·0 | 59·9 | 49·0 | 49·4 | 1813 | 37·6 | 47·6 | 57·9 | 46·9 | 47·7 |
| | | | | | | 1814 | 32·8 | 45·0 | 57·7 | 47·4 | 45·9 |
| 1751 | 37·8 | 45·6 | 58·9 | 47·1 | 47·2 | 1815 | 38·7 | 48·8 | 58·7 | 48·2 | 48·3 |
| 1752 | 37·6 | 45·6 | 59·6 | 50·6 | 48·5 | 1816 | 36·0 | 44·3 | 56·1 | 47·6 | 46·1 |
| 1753 | 38·0 | 47·4 | 59·3 | 48·6 | 48·2 | 1817 | 40·4 | 45·1 | 57·6 | 49·2 | 48·0 |
| 1754 | 38·3 | 45·4 | 58·4 | 50·0 | 47·9 | 1818 | 37·7 | 45·6 | 61·9 | 52·9 | 49·7 |
| 1755 | 36·2 | 46·0 | 59·2 | 48·0 | 47·4 | 1819 | 39·3 | 48·2 | 60·4 | 47·9 | 48·6 |
| 1756 | 39·7 | 45·0 | 58·7 | 51·5 | 47·8 | 1820 | 34·6 | 47·0 | 58·4 | 47·6 | 47·4 |
| 1757 | 36·3 | 46·2 | 60·6 | 49·2 | 47·9 | | | | | | |
| 1758 | 37·8 | 47·7 | 59·1 | 47·7 | 48·2 | 1821 | 38·2 | 46·8 | 58·1 | 52·4 | 49·1 |
| 1759 | 41·3 | 48·1 | 61·7 | 49·7 | 50·0 | 1822 | 42·5 | 49·3 | 60·7 | 50·8 | 50·1 |
| 1760 | 36·9 | 48·6 | 60·8 | 50·4 | 49·7 | 1823 | 34·8 | 46·3 | 56·4 | 48·8 | 47·1 |

| | Win. | Spr. | Sum. | Aut. | Year |
|---|---|---|---|---|---|
| 1824 | 40·3 | 45·6 | 58·7 | 50·2 | 48·7 |
| 1825 | 39·7 | 47·4 | 60·5 | 50·6 | 49·5 |
| 1826 | 38·8 | 47·8 | 63·7 | 49·4 | 50·1 |
| 1827 | 37·0 | 48·1 | 59·3 | 51·2 | 49·1 |
| 1828 | 42·3 | 48·4 | 60·0 | 51·1 | 50·5 |
| 1829 | 39·2 | 46·1 | 58·6 | 46·4 | 46·7 |
| 1830 | 34·0 | 49·2 | 57·6 | 49·5 | 47·7 |
| 1831 | 37·0 | 48·7 | 61·4 | 51·2 | 50·2 |
| 1832 | 39·4 | 47·2 | 59·9 | 50·1 | 49·1 |
| 1833 | 39·2 | 48·0 | 58·8 | 49·2 | 49·1 |
| 1834 | 43·7 | 48·7 | 61·1 | 50·7 | 50·8 |
| 1835 | 40·6 | 47·4 | 61·0 | 49·4 | 49·2 |
| 1836 | 38·2 | 46·4 | 59·2 | 47·3 | 47·9 |
| 1837 | 38·9 | 42·1 | 60·9 | 48·9 | 47·9 |
| 1838 | 34·5 | 44·9 | 59·1 | 48·3 | 46·5 |
| 1839 | 38·5 | 44·4 | 58·3 | 49·4 | 47·6 |
| 1840 | 38·8 | 47·0 | 58·3 | 46·6 | 47·3 |
| 1841 | 34·9 | 48·8 | 56·8 | 48·4 | 47·7 |
| 1842 | 37·5 | 47·4 | 60·3 | 47·5 | 48·6 |
| 1843 | 39·9 | 46·7 | 57·7 | 48·8 | 48·3 |
| 1844 | 39·7 | 47·2 | 58·2 | 49·0 | 47·5 |
| 1845 | 34·7 | 44·1 | 57·6 | 48·6 | 46·9 |
| 1846 | 42·4 | 47·7 | 62·8 | 50·7 | 50·3 |
| 1847 | 35·0 | 46·7 | 60·0 | 50·0 | 48·6 |
| 1848 | 39·3 | 48·8 | 58·2 | 49·0 | 49·0 |
| 1849 | 41·2 | 46·7 | 60·0 | 49·4 | 48·7 |
| 1850 | 38·3 | 46·3 | 59·6 | 48·5 | 48·4 |
| 1851 | 40·9 | 46·2 | 58·7 | 47·9 | 48·3 |
| 1852 | 40·7 | 46·4 | 60·6 | 49·1 | 49·6 |
| 1853 | 40·0 | 45·2 | 58·4 | 48·5 | 47·0 |
| 1854 | 37·5 | 47·7 | 58·3 | 49·2 | 48·8 |
| 1855 | 35·4 | 43·5 | 59·5 | 48·9 | 46·4 |
| 1856 | 38·8 | 45·1 | 59·4 | 48·8 | 48·5 |
| 1857 | 38·8 | 46·4 | 61·8 | 51·8 | 50·1 |
| 1858 | 39·6 | 46·1 | 60·5 | 49·2 | 48·5 |
| 1859 | 41·3 | 47·9 | 61·6 | 48·4 | 49·3 |
| 1860 | 36·1 | 45·3 | 56·3 | 47·3 | 46·2 |
| 1861 | 36·9 | 46·5 | 59·4 | 49·4 | 48·4 |
| 1862 | 39·8 | 48·0 | 56·9 | 47·9 | 48·5 |
| 1863 | 42·3 | 47·5 | 58·5 | 49·3 | 49·4 |
| 1864 | 37·0 | 47·6 | 58·0 | 49·1 | 47·9 |
| 1865 | 36·8 | 47·7 | 60·3 | 51·6 | 49·4 |
| 1866 | 41·6 | 46·0 | 59·4 | 50·3 | 49·4 |
| 1867 | 40·5 | 46·2 | 59·1 | 48·7 | 48·2 |
| 1868 | 40·2 | 49·4 | 62·4 | 48·6 | 50·7 |
| 1869 | 44·2 | 46·1 | 59·6 | 49·9 | 49·3 |
| 1870 | 37·4 | 47·3 | 61·0 | 48·3 | 48·2 |
| 1871 | 36·3 | 48·3 | 59·1 | 47·6 | 48·3 |
| 1872 | 41·3 | 46·8 | 59·8 | 49·1 | 49·5 |
| 1873 | 39·4 | 45·8 | 59·5 | 48·0 | 48·2 |
| 1874 | 40·8 | 47·9 | 59·8 | 49·8 | 48·8 |
| 1875 | 37·1 | 47·5 | 59·1 | 49·6 | 49·0 |
| 1876 | 39·3 | 45·2 | 60·8 | 50·1 | 49·1 |
| 1877 | 42·6 | 44·6 | 59·1 | 48·5 | 48·5 |
| 1878 | 40·9 | 47·7 | 60·7 | 48·2 | 48·6 |
| 1879 | 33·2 | 43·6 | 56·6 | 47·4 | 45·4 |
| 1880 | 36·5 | 46·7 | 59·5 | 48·2 | 48·4 |
| 1881 | 36·0 | 46·6 | 58·3 | 49·3 | 47·4 |
| 1882 | 41·1 | 48·4 | 57·9 | 48·6 | 49·0 |
| 1883 | 40·7 | 44·4 | 58·2 | 49·3 | 48·3 |
| 1884 | 41·8 | 47·0 | 60·5 | 49·5 | 49·7 |
| 1885 | 39·9 | 44·7 | 58·2 | 47·4 | 47·5 |
| 1886 | 36·4 | 45·3 | 59·2 | 50·9 | 47·7 |
| 1887 | 36·9 | 43·3 | 60·9 | 46·0 | 46·9 |
| 1888 | 36·6 | 44·0 | 56·6 | 48·7 | 46·8 |
| 1889 | 38·8 | 46·8 | 59·2 | 49·0 | 48·2 |
| 1890 | 39·2 | 47·0 | 57·2 | 49·8 | 47·7 |
| 1891 | 34·7 | 43·7 | 58·2 | 49·5 | 47·3 |
| 1892 | 38·0 | 44·9 | 57·7 | 47·5 | 46·7 |
| 1893 | 37·2 | 50·3 | 61·6 | 48·8 | 49·9 |
| 1894 | 40·0 | 47·3 | 58·1 | 49·3 | 48·7 |
| 1895 | 34·1 | 47·4 | 59·5 | 50·0 | 47·6 |
| 1896 | 40·0 | 48·7 | 60·0 | 46·6 | 48·8 |
| 1897 | 38·8 | 46·1 | 60·7 | 49·8 | 49·0 |
| 1898 | 41·7 | 45·8 | 59·2 | 52·2 | 50·1 |
| 1899 | 42·4 | 45·7 | 62·5 | 50·3 | 49·4 |
| 1900 | 37·5 | 45·3 | 60·1 | 50·5 | 49·2 |
| 1901 | 39·8 | 46·5 | 60·5 | 49·1 | 48·4 |
| 1902 | 37·8 | 45·8 | 57·8 | 49·6 | 47·9 |
| 1903 | 41·5 | 46·7 | 57·6 | 50·0 | 48·7 |
| 1904 | 38·5 | 46·4 | 59·3 | 48·5 | 48·2 |
| 1905 | 39·4 | 47·3 | 60·0 | 46·6 | 48·4 |
| 1906 | 40·0 | 45·7 | 60·1 | 51·3 | 49·0 |
| 1907 | 37·6 | 46·6 | 56·5 | 50·0 | 47·9 |
| 1908 | 39·4 | 45·6 | 58·8 | 51·3 | 48·7 |
| 1909 | 38·2 | 46·1 | 57·1 | 48·3 | 47·4 |
| 1910 | 39·5 | 46·7 | 58·5 | 47·8 | 48·5 |
| 1911 | 41·0 | 47·3 | 62·5 | 49·6 | 50·1 |
| 1912 | 41·1 | 48·8 | 57·8 | 47·3 | 48·9 |
| 1913 | 41·6 | 47·4 | 58·4 | 52·0 | 49·7 |
| 1914 | 41·3 | 48·0 | 59·8 | 50·2 | 49·8 |
| 1915 | 39·8 | 46·4 | 58·6 | 47·1 | 48·0 |
| 1916 | 42·0 | 45·8 | 58·1 | 50·2 | 48·5 |
| 1917 | 34·7 | 44·8 | 59·9 | 49·6 | 47·3 |
| 1918 | 39·6 | 47·3 | 58·9 | 48·0 | 49·1 |
| 1919 | 39·1 | 46·5 | 58·4 | 46·0 | 47·3 |
| 1920 | 42·0 | 48·3 | 57·9 | 50·1 | 49·2 |
| 1921 | 41·8 | 48·0 | 61·2 | 50·8 | 50·8 |
| 1922 | 40·8 | 45·7 | 56·6 | 47·7 | 47·6 |
| 1923 | 42·2 | 46·0 | 59·1 | 47·3 | 48·2 |
| 1924 | 39·1 | 45·6 | 58·0 | 50·3 | 48·7 |
| 1925 | 42·4 | 46·4 | 60·3 | 47·3 | 48·5 |
| 1926 | 40·5 | 47·5 | 60·1 | 49·0 | 49·5 |
| 1927 | 39·6 | 47·8 | 58·5 | 49·5 | 48·7 |
| 1928 | 39·8 | 47·4 | 58·6 | 50·3 | 49·3 |
| 1929 | 35·1 | 46·5 | 58·8 | 50·9 | 48·2 |
| 1930 | 40·3 | 46·6 | 59·7 | 50·2 | 49·0 |
| 1931 | 38·8 | 46·0 | 58·4 | 48·9 | 48·2 |
| 1932 | 40·7 | 45·3 | 60·3 | 49·0 | 49·0 |
| 1933 | 39·3 | 49·0 | 62·6 | 50·3 | 49·7 |
| 1934 | 37·7 | 46·5 | 61·1 | 50·7 | 50·0 |
| 1935 | 43·0 | 46·8 | 61·3 | 50·0 | 49·5 |
| 1936 | 37·5 | 46·9 | 59·6 | 49·6 | 48·8 |
| 1937 | 41·6 | 47·0 | 60·3 | 49·3 | 49·2 |
| 1938 | 40·3 | 48·4 | 59·5 | 52·2 | 50·3 |
| 1939 | 40·5 | 47·6 | 59·7 | 50·7 | 49·4 |
| 1940 | 34·6 | 48·3 | 60·2 | 49·6 | 48·3 |
| 1941 | 36·7 | 44·5 | 60·3 | 50·9 | 48·4 |
| 1942 | 35·9 | 47·2 | 59·9 | 49·4 | 48·3 |
| 1943 | 42·6 | 49·3 | 60·1 | 50·1 | 50·1 |
| 1944 | 39·7 | 48·0 | 60·2 | 48·8 | 49·2 |
| 1945 | 38·6 | 50·1 | 60·3 | 52·1 | 50·6 |
| 1946 | 40·2 | 47·8 | 58·4 | 51·1 | 49·0 |
| 1947 | 34·0 | 47·4 | 62·7 | 51·6 | 49·2 |
| 1948 | 41·1 | 49·2 | 58·6 | 50·7 | 50·0 |
| 1949 | 42·1 | 47·7 | 61·7 | 52·8 | 51·1 |
| 1950 | 41·2 | 47·8 | 60·6 | 48·9 | 48·9 |
| 1951 | 37·3 | 44·6 | 59·1 | 51·2 | 48·7 |
| 1952 | 39·0 | 49·8 | 60·2 | 46·1 | 48·4 |

TABLE 2

| | Winter | | Spring | | Summer | | Autumn | | Year | |
|---|---|---|---|---|---|---|---|---|---|---|
| Warmest | 1868/9 | 44·2 | 1893 | 50·3 | 1826 | 63·7 | 1730, 1731 | 53·2 | 1949 | 51·1 |
| Coldest | 1739/40 | 31·3 | 1837 | 42·1 | 1725 | 55·6 | 1786 | 45·5 | 1740 | 44·3 |

TABLE 3

| J | F | M | A | M | J | J | A | S | O | N | D | Year |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 39·1 | 39·5 | 42·7 | 46·9 | 52·3 | 57·9 | 61·3 | 60·5 | 56·5 | 50·0 | 43·5 | 40·0 | 49·2 |

exceptionally strong and bitter easterly wind all day, the third in succession on which it had been blowing from the Continent. In recent years there was a screen maximum of 26°F at Plymouth with a similar very strong wind on 29 Jan. 1947. Borlase's reports of the damage to his garden near Penzance bear out the severity of this ferociously cold gale (29 to 31 Dec. O.S., 1739) when on the 31st for many hours the east wind blew in Holland with a temperature below zero, and I surmise that on the same afternoon in London it was about, or even below 15°F. The severity of the gale was such that post-boys are stated to have perished of cold on horseback; in Rutland three inches of ice formed within 24 hr; and ships could not be worked in the Thames estuary (*Phil. Trans.*, **58**, p. 54, 1768 and *G. Mag.*, pp. 16 and 59, 1754). Symons probably did not appreciate that the severity of cold was due to the strong wind, which would be far worse to the senses than the below-zero readings recorded in the London area a few days afterwards; it may well represent the *ne plus ultra* of Continental-arctic air in England. I quite agree that indoor readings would be welcome for comparison with outdoor means. Some years ago, by way of interest, I kept ' deliberately bad ' indoor observations of my own for upwards of a year at Cambridge, following a cheerful suggestion by Sir Ronald Fisher. Barker at Lyndon and Bent in Paternoster Row also kept both indoor and outdoor readings in the late 18th century, and the pattern of Barker's readings (*Phil. Trans.*, 1771-1798) gives a very fair illustration of what happens. I found that my bad readings at irregular hours at Cambridge when reduced to monthly means made a pattern sufficiently similar to inspire me with reasonable confidence in the adoption of reductions for older indoor readings, provided that one had daily observations of the weather with which to confirm events. Even in a draughty bedroom with the window closed day-to-day lag is appreciable : but over a month these minor variations largely smooth out. Unfortunately I have now lost these records, but I will keep my eye open for others following Mr. Hawke's suggestion. I believe there are some at Eskdalemuir. To illustrate the pattern; over the years 1771-1786 the mean differences between Barker's indoor and outdoor readings, taken early in the morning and in mid-afternoon, were as follows (Jan.-Dec.) : 3·6, 3·0, 2·8, 2·0, 0·8, 0·1, 0·0, 1·4, 3·0, 4·3, 4·8, 3·9; the outdoor being the lower, except in July. The latter, of course, are only very roughly approximate to normal monthly means as they would need in any event to be further corrected for the hours of observations.

Continuing in further detail with regard to Dr. Glasspoole's contribution : The fundamental assumption made by Dr. Glasspoole appears to be, that over any period of time, fluctuations have always been about the same mean. He begins by implying that his method of statistical study of temperature variations for the last 50-100 years, using the standard deviation to test records for homogeneity, is to be preferred. This is utterly untenable. Incidentally, I have been unable to find where he has advocated such a method in print or what criticisms were then made. With regard to the applicability of a standard deviation over longer periods I wonder if he has ever paid any attention to the secular variation of temperature or its amplitude, which has been the subject not merely of European but of world-wide discussion for many years ? Moreover, in the light of existing knowledge and material available, his suggested method would be not only inappropriate but clumsy. Scrutiny of any of our longer records will quickly show that changes in the type of screen, in the method of deriving a monthly mean, and more particularly in the hours of reading and setting thermometers are all likely to give rise to discontinuities, and that such changes, which must all be allowed for, are as a rule entered in the actual journals.

Common sense would therefore lead many to look directly for the evidence. Dr. Glasspoole proceeds to enumerate the variations which he says are hidden in any one of my mean values. He must surely be aware that the acceptance of many of the mean values printed in the *Monthly Weather Report* can at once be questioned on the ground of changes in the hours of observation, let alone several other difficulties; whereas the basic Oxford and Lancashire records on which I built have at least the merit of having been reasonably standardized.

He then remarks that each part of central England has its own distinct variation with time. Such a statement has a limited application in certain places, notably in large and growing cities such as Nottingham and Birmingham; here I would also remind him of Mr. Callendar's comments (*Q.J.*, p. 265, 1952). But it certainly cannot apply to all parts of the region; unless qualified, such a statement suggests a very odd view of meteorology and of the capabilities of the air masses to which we are subject. It leads one to wonder what purpose if any has ever been served by the numerous observers who have prided themselves on maintaining both continuity and strict homogeneity in unchanging rural situations over long periods. Dr. Glasspoole would presumably prefer to amalgamate the results of their labours with the less trustworthy city records in some form of collected table or map, a procedure with which I heartily disagree. Indeed I may add that the present use of Nottingham (Castle) and Birmingham (Edgbaston) as two of the five stations in the compilation of the M.O. ' District 4 ' or ' Midlands ' mean has in principle nothing to commend it; fortunately it is customary at present merely to quote anomalies and the results over a short term of years are effective enough.

It is indeed a pleasure to learn that the correlation coefficient between annual means at York and Plymouth proves to be less than unity. After all they are more than three degrees of latitude apart and one is adjacent to a sea. I should have thought this would be obvious, and hardly to be acclaimed as a triumph. Neither do I see the relevance of the remarks about the very obvious fact that the variability increases to the eastward; what I have looked for is the variability in the middle, whether by months, seasons or years, using stations on either flank and starting as I have already said with long and well-standardized tables of monthly means. His results with regard to the monthly variability at Plymouth and York are also quite obvious and do not appear relevant to the discussion which is concerned with the likelihood of events in the west Midlands; his attribution to ' the chance of cold air ' leads one to ask whether he has ever examined the variability in, for example, the Shetlands.

Altitude and reduction to sea-level are quite irrelevant issues, and I am grateful to Mr. Schove for commenting on this. Surely it is obvious that if Oxford means derive from a station 208 ft above sea-level and not subject to local frost-hollow effects, while Lancashire means refer to similar inland locations near Preston about 50 ft above sea-level, the mean of the two is likely to be representative of intermediate rural locations; that is, with a medium soil, neither hill-top nor frost-hollow, at 100-150 ft in the West Midlands. I hope I have made it clear in my paper that emphasis on intermediate locations is necessary on account of the fact that a Shropshire station such as Newport is obviously subject to additional local effects which are at once revealed by comparison with Shrewsbury, Cheltenham or Wrexham; most workers can read a map.

I did not choose to standardize my figures on to those of the Meteorological Office ' Midlands,' or ' District 4,' on account of the fact already mentioned that in recent years two city stations have figured among the five from which means were derived; among the three others is Oxford, although the Office does not appear to use Mr. Balk's superior reduction. Neither are the maps, which Dr. Glasspoole proposes I should use as a basis for the derivation of mean temperature, sufficiently reliable. I sampled those for 1906 and found the isopleths crudely drawn. Sometimes use was then made of stations with peculiar characteristics such as Garforth; isopleths were drawn right across the Pennines without any supporting station; no reference appears to have been made to the effects of observing hours. Therefore I refute in the strongest terms the suggestion that these maps would give ' better values ' for central England. Dr. Glasspoole's judgment is here in my view thoroughly faulty. Incidentally if he prefers the term west Midlands he can have it, but I have not wished to risk needless confusion with the existing Meteorological Office nomenclature; neither is it right to name any specific county. I take the opportunity to challenge Dr. Glasspoole to tell me how the map for January 1953, a month of remarkable local anomalies, can be expected to give anyone a better representative value than that provided by my method (38·0°F).

I had no necessity to refer to the 1942 paper. In it Dr. Glasspoole with his collaborator Mr. Hogg give a series a monthly means at sea level for England and Wales since 1901, a mere forty years, using the highly questionable isopleths of the older *Monthly Weather Report*. It surprises me that Dr. Glasspoole should advocate my ' continuing to earlier times one of the

series included in that paper ' – presumably the figures for England and Wales as a whole – in the light of his earlier remarks about variability. He would be well advised to consider what would happen in a month such as Dec. 1890, and to decide what balance of coastal, inland and upland stations under the then conditions of observation would give a mean representative of the whole of England and Wales, and what use such a figure would have for European climatologists when he got it. I prefer to discuss the ' central England ' region I have named, for after all the Radcliffe tables for Oxford have the merit not only of being very representative of the southern Midlands but also of being standardized back to 1815 – eighty-five years beyond the mere forty that Dr. Glasspoole would suggest I should use. Further we do know that the results apply to an inland location and are not, like his, an uncertainly-balanced aggregation of coastal, inland and upland effects.

Moreover the values in my paper can readily be continued. It will be entirely reasonable within the inherent limits of instrumental and other errors to use one or more of several representative south Midland stations at which the records overlap the Radcliffe means; that is the whole point of my method which bears obvious resemblance to the principle of resection by which a surveyor can establish his position from three known points. Indeed, as Oxford is now becoming more urbanized, it may be preferable to use other stations in future and I sincerely hope that Dr. Glasspoole will not regard a mere year as sufficient overlap when he considers the merits of a new ' Oxford ' location. Abingdon and Cheltenham combined may well commend themselves. It may be added here that I have tested the Oxford annual means since 1935 and find no appreciable effect as yet when comparison is made with other stations.

In the light of my earlier comments it will be appreciated that Dr. Glasspoole's remarks about the ' close qualitative accord ' with the data in his 1942 paper strike me as somewhat presumptuous, neither can I feel that they represent any compliment to the careful work of the Radcliffe observers. His scheme is unlikely to lead to serious error, taken over the country as a whole, but I should much doubt it for individual districts. Is he aware, for example, that Midland district means formerly incorporated those from the old low-lying Nottingham station by contrast with the present Castle ? With regard to providing the figures for each station used throughout 255 years, they are all printed back to 1815; and he will find my reductions of 40 years of Cary's observations (1786-1825), not previously worked out, at the Society. Beyond that, I could indeed have used space in this *Journal* with twenty papers on a miscellany of older records and it seemed to me more important to assume that most readers dislike fragments and are acquainted with method. I have no objection to providing such figures but must plead that, lacking the resources of an office, of adding machines, or of secretarial help with which to tabulate the separate elements in five years' work, much of which covers manuscripts in libraries to which I have been given privileged access, I have instead given all the sources and hope that others will see their way to do the necessary arithmetic and provide further corroboration of the earlier values. There are still many neglected older records in the library of the Meteorological Office which will repay examination and before adding further material from privileged sources I much hope that Dr. Glasspoole will himself set about the problem of standardizing some of them in order to escape from the bondage of 1881.

I shall not pretend that my table of monthly means can be any more than an imperfect effort; such means can by their nature not be precisely applicable to any one location and I have no desire to exaggerate their merits. They are, on the other hand, likely to be reasonably representative of what has happened in the past 255 years in central England and can be provisionally used as a basis for discussion of the incidence of other meteorological phenomena. When Dr. Glasspoole can provide properly standardized monthly means for Nottingham (1810), Reading with Wycombe (1823), Wakefield (1847) and Belvoir Castle (1855) to name only four stations with commendable records which are I believe available to him in the M.O. library, we shall be in a much better position to judge the Midland vicissitudes; and I venture to say that, unless major errors have been overlooked in the Oxford reductions, no serious difference in *trend* from that shown in the present table will be revealed. If having done that he will then devise a better means of extension to include the 18th century Midland records, I shall be very ready to welcome the result; for we need it. In principle there is nothing wrong with Dr. Glasspoole's methods of deriving means, given caution, by using the maps, but the imperfections in the existing official records since 1881 do not lead me to feel that they should be used for specific regions without much greater care than he appears ready to give. I note his reference to rainfall; I do not however consider myself competent to enter as yet into the question of method in respect of that element.

# Proceedings at meetings of the Society

*Special General Meeting*, 17 *June* 1953

A Special General Meeting of Fellows was held to elect the Officers and Council for the year 1953-4.

Fellows had been circulated on 10 April with the list of nominations for Officers and Council for the year 1953-4. No other nominations having since been received, the persons so nominated were declared to be elected unopposed in accordance with By-Law 51.

The Officers and Council for 1953-4 were therefore elected as follows :

PRESIDENT

Professor OLIVER GRAHAM SUTTON, C.B.E., D.Sc., F.R.S.

VICE-PRESIDENTS

Professor PERCY ROBERT CROWE, PH.D.
SIR CHARLES NORMAND, C.I.E., M.A., D.Sc.
CAMERON DARRELL OVEY, B.Sc., F.G.S.
Professor PERCIVAL ALBERT SHEPPARD, B.Sc., F.INST.P.

VICE-PRESIDENT FOR CANADA

FRANK WALDEMAR BENUM, M.A.

VICE-PRESIDENT FOR SCOTLAND

ALEXANDER EBENEZER MCLEAN GEDDES, O.B.E., M.A., D.Sc.

TREASURER

JAMES STRACHAN FARQUHARSON, M.A., D.Sc.

SECRETARIES

HOWARD LATIMER PENMAN, M.Sc., PH.D., F.INST.P.
GEORGE DAVID ROBINSON, PH.D., F.INST.P. (Librarian).
RICHARD GEORGE VERYARD, B.Sc.
THOMAS WILSON WORMELL, M.A., PH.D. (Editor)

COUNCILLORS

GEORGE ARTHUR BULL, B.Sc.
HENRY CHARNOCK, M.Sc., D.I.C., A.INST.P.
DONOVAN CHILTON, B.Sc., A.INST.P.
Instr. Cdr. CHARLES ROY DARLINGTON, R.N., B.Sc.
Mrs. ANN COURTENAY DOUGLAS, M.B.E.
ARTHUR GEORGE FORSDYKE, PH.D., D.I.C., A.R.C.S.
RONALD FRITH, O.B.E., PH.D.
Lt. Cdr. GEORGE LAWRENCE HOGBEN, R.N.Z.N., D.S.C., M.A., D.I.C., PH.D.
RALPH EDWIN LACY, B.Sc.
FRANK HENRY LUDLAM, D.I.C.
NORMAN PYE, B.A.
RICHARD SEGAR SCORER, M.A., PH.D.

*Ordinary Meeting*, 17 *June* 1953

The following candidates were declared to have been elected that day by the Council :

FELLOWS

BAHR, Victor John, World Meteorological Organization, Avenue de la Paix, Geneva, Switzerland.
DIXON, Eric Hubert, B.Sc., 15 Ramsey Terrace, Otley, Yorkshire.
KIME, Flt. Lt. John Gelsthorpe, R.A.F.R.O., 2 The Pingle, Derby Road, Long Eaton, Nottingham.
MAHONY, George Francis, 58 Tudor Way, Hillingdon, Middlesex.
PHILPOTT, Instr. Lieut. David R. M., R.N., B.A., 32 Golds Hill Road, Handsworth, Birmingham 21.
SIVASWAMI, Gangaikondan Venkatasubramania, A.M.AE.S.I., Administrative Officer, Operations, Air India International Ltd., Bombay, India.
THICKE, James Lionel, 58 Greenbank Avenue, Plymouth, Devon.
WOOTTON, Frederick Percival Leonard, 22 West Street, Ventnor, Isle of Wight.

FOREIGN MEMBERS

CHANG, Hung Chun, 124 Boundary Street, Kowloon, Hong Kong.
KESSLER III, Edwin, 369 Westgate West, Cambridge 39, Mass., U.S.A.
LANGLO, Kaare, Chief of Technical Division, W.M.O. Secretariat, 1 Avenue de la Paix, Geneva, Switzerland.
MALKIN, William, 8120 14th Avenue, Apt. 201, Hyattsville, Maryland, U.S.A.
SWOBODA, Gustav, D.PH., Secretary-General, World Meteorological Organization, Avenue de la Paix, Geneva, Switzerland.

ASSOCIATE

PADYA, Brahmanund Mohabeer, Meteorological Office, Vacoas, Mauritius.

Capt. L. R. Ambrose, London Manager of Qantas Empire Airways Ltd., gave a short talk on ' Indian Ocean Operations.'

Sir Charles Normand, C.I.E., M.A., D.Sc., then delivered the Presidential Address, his subject being ' Monsoon seasonal forecasting.'

Mr. J. S. Sawyer proposed a vote of thanks to the President for his Address and asked that he would allow the Address to be printed in the *Quarterly Journal*. He said he thought this India experiment was a model of caution and self-criticism.

In seconding the vote of thanks, Instr. Cdr. C. R. Darlington, R.N., thanked the President for his great help to the Royal Navy during his work in India.

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*Summer Meeting*, 15 *July* 1953

The Summer Meeting of the Society was held on 15 July 1953 at the Building Research Station, Garston, near Watford, by courtesy of the Director. Some 50 Members and guests attended and enjoyed a very interesting and instructive afternoon. A full account of the meeting was published in the August number of *Weather*.

# Reviews

*Hilfsbuch für raum und aussenklimatische Messungen.* By F. Bradtke and W. Liese. Berlin (Springer-Verlag). 1953. Pp. 108; 37 Figs., 12 tables. 15 DM.

This is a small treatise on human bio-physics, dealing mainly with the reactions of the human body to its meteorological environment. Rather more than one-half of the text is devoted to methods of observation of temperature, humidity and wind, and the representation of meteorological conditions by special concepts such as effective temperature, which is presumed to give as a single factor the joint effect of temperature, humidity and air motion, or special instruments such as the katathermometer, whether wet or dry.

The heat balance of the body, as affected by internal and external factors, is discussed briefly in the first two sections of the book, while establishment of special criteria for comfort, in various degrees of activity indoors, is rather briefly discussed in a final section of some eleven pages.

An appendix gives psychrometric tables, vapour pressures of water, mean and absolute yearly minima of temperature for winter and summer, yearly frequency of days of ice, frost, summer conditions (maximum temperature above 25°C), clear and cloudy days, for a number of German cities, and several other useful tables.

To summarize the book would be an impossible task, since the book itself is a closely packed summary of factual data, and of the physiological principles to be considered. It is not a book to be skimmed over lightly. It needs careful and close study to master the very large amount of information with which it is filled. The present reviewer has no serious criticism of the content of the book, which will repay the effort which must be made to follow in detail the matter compressed into its pages.

D.B.

*Essentials of fluid dynamics.* By L. Prandtl. London (Blackie and Son), 1952. Pp. x, 452; many figures. 35s.

Most textbooks of fluid dynamics have been written by – possibly to some extent for – mathematicians. Professor Prandtl's book is quite different. It is a discursive, selective, at times opinionated introduction and guide to fluid dynamics written by a first-rate physicist. In it, to quote the author's preface, 'in general, formulae take second place, the principal object being the awakening of clear intuitive apprehension.'

The book contains chapters on hydrostatics, on the dynamics of frictionless and of viscous fluids and on flow with appreciable volume changes, together with a long chapter containing sections on cavitation, flow in rotating systems, flow in heavy, stratified fluids and on heat transfer. There are many references to experimental results and to practical applications. Meteorology is mainly confined to one section in the final chapter.

Prandtl's treatment is always satisfying from a physical standpoint but, possibly owing to delay in translation and publication, not always up-to-date. There is, however, an appendix of notes and references to papers in English; the latest one referred to is dated 1950.

Many meteorologists and oceanographers will find this a stimulating companion to the classical, highly mathematical texts.

H.C.

*Handbook of statistical methods in meteorology.* M.O.538. By C. E. P. Brooks and N. Carruthers, London (H.M.S.O.), 1953. Pp. viii, 412; 75 Figs., 128 tables, Appendices. 25s.

Many meteorologists are understandably nervous of a statistical approach to meteorological research, fearing that the physical aspects of a problem may speedily be lost in masses of numbers. Yet even the most elegant of theories requires verification in the hard light of observed facts, and it is therefore essential that meteorologists be conversant with at least the basic principles of statistical methods. This they have not always been. A perusal of meteorological literature provides little evidence of substantial change since W. H. Dines wrote in 1915, 'Perhaps no science has suffered so much from the want of what may be called statistical common sense as meteorology.'

In many cases there has been a failure, so forcibly condemned by Jeffreys, to give a 'properly determined standard error,' a failure which usually leads, perhaps mistakenly, to the assumption that the 'sample' characteristics are those of the 'population': the activities of the type of meteorologist quoted by Sir Gilbert Walker, as having 'ceased to pay attention to criteria (of periodicity significance) because when he did so he obtained scarcely any results of interest' have unfortunately not been confined to period-finding. (The reviewer would emphasize that he writes not as an expert but as a convert!). In other cases authors have not appreciated some more subtle statistical point: for instance, some of the insidious statistical effects of persistence of meteorological events have been recognized since at least the 1930's but still take a heavy toll of research conclusions.

This book is the result of the recognition by the authors of the need for adequate methods of analysing large quantities of observational data – accumulating, surely, more rapidly in meteorology than in any other branch of physical science. The work divides conveniently, though not rigorously, into three Parts.

Part I concerns those methods of analysis and representation of the individual meteorological elements best calculated to reveal their essential features. The frequency distributions of the elements are also discussed in relation to the important types of theoretical distribution. The concept of probability is introduced early,

and some important tests for significance are described : the practical use of the tests is amply illustrated throughout the book. Part I ends with a chapter devoted to the analysis of variance of a series of observations.

Part II is concerned in the main with the relations between variables and with allied subjects. It opens with a description of the use of contingency tables and of convenient methods of diagrammatic representation. This leads to a notable chapter on vector quantities, containing a timely and most useful account of methods of analysing wind observations. There follow chapters on simple and partial correlation and on curve fitting. Included in this section also, apparently for want of a more convenient place, is a chapter on various statistical procedures of practical importance – smoothing, weighting, interpolation, etc. A place in this chapter might conveniently have been found for a discussion of some known statistical ' traps ': the too-ready assumption that a smooth progression of running means implies a real secular change; the misleading effects of curvature often associated with a choice of certain conditions; the special selection of a certain sequence of events and subsequent test for significance by comparison with the expectations based on random choice.

The authors' treatment of correlation calls for some comment. Nowhere do they emphasize that $N$ in the test for significance of a coefficient is the number of *independent* pairs of observations correlated. In practice this is often only a small fraction of the total number of pairs correlated. We find, in fact, that in their example illustrating bi-serial correlation, significance is tested on the assumption that $N$ is the total number of correlated pairs in spite of the fact that these were values of rainfall and surface pressure relating to successive days. Tests applied on such a basis must lead to some relations being wrongly considered real.

That feature of weather termed here ' persistence ' – alternative names are auto-correlation, conservation, coherence – is so often altogether ignored that it is a relief to find that an entire chapter, the first of Part III of the book, is devoted to its study. The authors are however plainly not at their happiest in this chapter. They point out that their measure of the effect of persistence on frequency distributions differs from that of Baur; and in Appendix XI they note a corresponding discrepancy between their persistence factor for a continuous variate and that given by Bartels. The discrepancies are far from negligible, since the measures differ by a factor of nearly 2. The persistence factor of Brooks and Carruthers seems to imply unresolvable difficulties in the analysis of the variance of means of a coherent series, difficulties which do not arise with the definition of Bartels. (It is, incidentally, surprising to find no other reference in the book to the work of Bartels, who has done perhaps more than anyone to put the analysis of geophysical and meteorological series on a sound basis.)

Six chapters devoted to a study of periodicity comprise the remainder of the book. They include a description of the harmonic-analysis process and of periodograms based on harmonic analysis and other methods : an interesting comparison between the relative efficiencies of the various methods is made by means of a test series. Tests of significance are possible only with the results obtained from harmonic analysis and the authors have, apparently, here ignored the important practical effects of persistence. Thus, though they point out that persistence is liable to lead to quasi-periodicity, and that this is especially true of data smoothed by moving averages (persistence being then artificially increased), their significance test is based on purely random numbers and they give no indication that a more stringent test is required to take account of any auto-correlation present in the series. Considering the vast amount of quite fruitless periodicity research there has been in meteorology, one wonders whether the authors have not dealt more fully with this subject than it deserves; they have certainly left the door too wide open for the future discovery of ' significant ' periods. Kendall is in little doubt on this subject : ' for economic series, and probably for meteorological and geophysical series as well, periodogram analysis is simply not worth the trouble of the arithmetic which it involves.'

The apparent short-comings of the work mentioned in the course of this review can hardly be considered trivial because they closely concern some of the commonest sources of error in the statistical aspects of meteorological research. They comprise, however, only a small section of the whole work which deals with the subject more comprehensively than has hitherto been attempted, at least in English. Every meteorologist will find much that is interesting and useful in this book. The exposition is clear, and the work is illuminated throughout by illustrative examples. An unexpected but valuable feature is the inclusion of examples to be worked at the end of most of the chapters. The appendices contain a number of very useful formulae and reference tables. There can be no doubt that this is an outstanding addition to meteorological literature and will be a standard reference book for years to come.

D.H.M.

*The upper atmosphere.* By S. K. Mitra. Calcutta (The Asiatic Society), 2nd Edition, 1952. Pp. xxiv, 713; 326 Figs., many tables. 48 r.

Although meteorologists are becoming more and more interested in the higher levels of the atmosphere, work on conditions above the stratosphere still remains mainly the province of the geophysicist and the radiophysicist, and this book is written from their point of view. It contains, however, much material on the stratosphere and the ozone content which is of direct meteorological importance, and everything in it must be useful and stimulating reading for meteorologists who wish to widen their range of interests. Many will already be familiar with the first edition.

The main changes in the second edition consist in the addition of a completely new chapter on rocket exploration of the upper atmosphere, and in extensions and revisions of many of the sections. The main

changes have been made in the sections dealing with the radio studies of meteors, the ozone content, the dissociation of molecular nitrogen, tides, winds and travelling disturbances, the theory of magnetic storms and the temperature distribution. One feels, perhaps, that the revision has not always been as thorough as might be desired. For example, in Chapter XI (p. 553), the results of a recent review by Gerson is quoted as supporting a temperature of 2,000°K to 3,000°K at 300 km height, and a collision frequency of electrons determined mainly by collisions with neutral atoms and molecules. On the other hand, the discussion of collision frequencies in Chapter VI (p. 261) is left as in the first edition, and the temperature chosen there is 1,000°K. Now reference to the curves in Gerson's paper shows that at this temperature the collision frequency would be increased by the effect of collision between electrons and positive ions, and as a result would be considerably above the observed value. Since the book is essentially a compilation of the views of many workers, it is perhaps too much to expect complete consistency. As the author himself remarks, there must inevitably be omissions due to ' the accumulation of facts and data too rapidly to be properly systematized.'

In spite of these inevitable limitations, the book is, and will remain, the standard work in its field. It is indispensable to those actually engaged on subjects coming within its scope, if only for the remarkably extensive bibliography, which contains more than 800 references. Others will find it a stimulating, if not always infallible, guide to a rapidly advancing and expanding field of study.

B.H.B.

*Barometrographia*

This Society is indebted to the courtesy of its elder brother the Royal Astronomical Society for the recent gift of a copy of Howard's *Barometrographia*, presented by the author in 1848. Luke Howard, F.R.S. (1772-1864), a prosperous commercial chemist and a close friend of John Dalton, is often known as ' the father of British meteorology.' He is famed for having originated the system of cloud classification and nomenclature which, with amplification, has since come into world-wide use. For this pioneer activity he enjoyed the distinction of having a poem written in his honour by Goethe (*Howards Ehrengedächtniss*). *Barometrographia*, which bears the explanatory sub-title *Twenty years' variation of the barometer in the climate of Britain exhibited in autographic curves, with the attendant winds and weather and copious notes illustrative of the subject*, was described by the late Sir Napier Shaw in his *Manual of Meteorology*, as ' a book of some magnificence.' In one sense at least it was Howard's *magnum opus*, for the two constituent volumes, published through the London firm of R. and J. E. Taylor in 1847, each measure about 27 in. by 21 in. and weigh several pounds. This generosity of bulk appears to have occasioned difficulties with transport, for although the Royal Astronomical Society's copy is entered as having been presented by Howard (who then resided at his country seat in Yorkshire) on 16 March 1848, it was not received in the library until nearly three months later.

The essential feature of the work is an engraved series of circular coloured diagrams, one per page, showing within their peripheries the changes of atmospheric pressure and its departure from an assumed normal day by day throughout every year from 1815 to 1834, as recorded by a mercurial barograph. These annual diagrams also include curves giving the solar and lunar declination, as well as the dates of the moon's phases, apogee and perigee. The space inside the circles is occupied by printed monthly totals of rainfall, means and extremes of temperature, and summarized accounts of contemporary winds and weather. The barograph had been made as an adjunct to an astronomical clock soon after the middle of the 18th century by the mechanic Alexander Cumming, who used it intermittently from 1765 until he died at the age of 81 on 8 March 1814. Registration was effected by means of a pencil attached to a float in the short leg of a siphon barometer, marking the changes in the level of the mercury on a circular chart revolved by the clock-work. After Cumming's death Howard purchased the instrument, and by keeping it as best he could in constant action from 1815 to 1834 he achieved what appears to have been the earliest known autographic record, maintained with a near approach to continuity, of a meteorological element over a 20-year period.

The main aim of *Barometrographia* was to provide the means of judging the relation between pressure changes and weather.Howard's reason for introducing the lunar phases and positions into the annual diagrams was his belief that the moon exerts an appreciable influence on the atmosphere ' through the medium of the winds, if not also directly.' He reached this conclusion on inadequate grounds – an analysis of data extending over only two complete years, one of them wet, the other dry – and in evident unawareness of the length to which coincidence can go in these matters. An extreme example of that length may be cited from the Greenwich Observatory records at the close of the 19th century. In each of the 12 consecutive years 1889 to 1900 there was a substantially greater amount of precipitation during the three days centred at new moon than during the third, fourth and fifth days after the incidence of full moon, the aggregate fall of the former periods exceeding that of the latter periods over the whole 12 years by as much as 74 per cent. This seeming support for the contention put forward by Theophrastus in the fourth century B.C. that the beginnings and ends of lunar months are apt to be stormy is shown by the Greenwich statistics to have been absent before 1889 and to have died out in 1901. Today meteorologists who have given attention to the subject appear to be in almost unanimous agreement with the verdict which the late W. J. Humphreys reached after investigating mathematically the three possible modes (gravitational, thermal and magnetic) of lunar action on the earth's atmosphere : ' Except indirectly through the tides, and that locally and very slightly, the moon cannot and does not measurably affect the weather in any respect whatsoever.'

Careful study of *Barometrographia* in conjunction with Howard's earlier work *The Climate of London* (1833) would be well worth while and may be recommended to Members of the Society who would like to advance knowledge of British weather trends during the few decades immediately preceding the introduction of synoptic meteorology. The period covered by Howard's observations is of considerable interest, containing as it did the notable summer drought of 1818 and one year, 1816, which on account of its abnormal cold over most of the northern hemisphere (attributed by some later writers to the loading of the upper atmosphere with volcanic dust and ashes ejected during the great eruption of Tomboro, Sumbawa, in April 1815) became known in the United States as ' Eighteen-hundred-and-froze-to-death ' and in Britain as ' The year without a summer.'

Cursory examination of the *Barometrographia* diagrams shows that in 1818 pressure exceeded the average level in the London district almost continuously from mid-May to late August, and suggests that there was a persistent extension of the Azores anticyclone over southern England, as in 1921. It may also be deduced that the exceptional severity of the frost early on 9 February 1816, when Howard's sheltered thermometer at Tottenham marked — 5°F, was the result of intense radiation from a deep snow-cover under clear skies. An active depression and cold front appear to have moved south-eastward across the English Channel to northern France on 7 February. That day gave Tottenham a heavy snowfall, strong NE. winds, and a maximum temperature of 31°F after a minimum of 15°F. Paris, which was then in the warm sector of the depression, had a maximum of 49°F on 7 February, and from the fact that the minimum there on the morning of 9 February was 18°F (23°F higher than at Tottenham) it would seem that the French capital had escaped appreciable snowfall. These few points are mentioned to show the type of working material that meteorologists with antiquarian interests may exhume from Howard's engaging volumes.

E.L.H.

*The British Caribbean Meteorological Service. First Annual Report.* Trinidad (Government Printing Office). 1953. Pp. 23. 24 c.

At a conference in Trinidad early in 1951 it was agreed that a unified British Caribbean Meteorological Service should be formed under a Director who would be directly responsible to the respective local Colonial governments. Mr. W. A. Grinsted was appointed Director towards the end of 1951 and this is his first annual report.

The Report is in four parts. The first part gives a summary of the meteorological facilities in the British Caribbean Area on 1 April 1951 when the new Service became responsible for its own finances and the second part refers to the work done since the formation of the new Service. Part III provides details of the internal organization of the Service whilst Part IV is a brief conclusion.

As is the case with so many meteorological services the Director found that his main problem was that of obtaining adequate staff and he is to be congratulated on being able to secure authority for instituting a training course. Reference is made in the Report to plans for raising the standard of observations, to participation in the hurricane service in the area and to the provision of increased facilities for aviation. In fact, it would appear that this new meteorological service has made a good start and its progress, as revealed in future Annual Reports, will be watched with interest. May we wish it every success ?

R.G.V.

*Das Wetter und seine Ursachen.* By H. J. aufm Kampe. Darmstadt (Dietrich Steinkopff), 1953. Paper covers. Pp. viii, 164; 129 Figs., 7 tables. DM. 29.80.

In the preface to this book the author states that his purpose in writing it was to summarize the advances in synoptic meteorology over the last two decades and to present an objective appreciation of the methods used at various forecasting centres. It will therefore be with a sense of disappointment that, on perusal by the professional meteorologist, the book will be found to be a textbook of synoptic meteorology such as might have been written at about the end of the second world war. It covers a similar field to that of a number of the existing textbooks. The early chapters, dealing with the pressure-height relation, the relation between wind and pressure distribution, vertical stability, fronts and air masses call for little comment. The treatment is mathematical, not altogether elementary, and as far as the reviewer can judge, sound. A lengthy chapter deals historically with the formation and evolution of extra-tropical depressions, from the early ideas of Bigelow, Shaw and Exner, through the work of the Norwegian school to that of Scherhag and Räthjen. Of the more recent application of atmospheric dynamics to synoptic meteorology there is no mention. The later chapters include brief references to Weickmann's symmetry points, singularities, a little cloud physics and some miscellaneous phenomena such as fog and thunderstorms. The book concludes with a lengthy list of ' Rules for forecasting.'

The reviewer found little new in this book but it can be recommended to students as covering an essential part of the meteorological curriculum while providing useful practice in reading scientific German.

A.G.F.

*Reviews of research on arid zone hydrology.* UNESCO (Paris), 1953. Pp. 212; Figs., tables. 25s.

Originally prepared for the guidance of the Unesco Advisory Committee on Arid Zone Research these eight reports are now made generally available in a single volume, obtainable from H.M.S.O. In order, the reports deal with : north-west Africa; north-east Africa, south Africa and Angola, the Middle East, India and Pakistan, U.S.A. and Canada, Latin America and Australia. The authors were asked to give particular attention to underground water and fluid mechanics, and with varying degrees of emphasis and detail they have provided surveys of current research on the source, quantity, quality and fate of underground water in their regions. Each survey has a long and useful bibliography. After the reports is an article on ' World distribution of arid and semi-arid homoclimates ' with maps based on Thornthwaite's method of classification.

The subject matter of all the reports is in the borderland of meteorology, geology and engineering. There is almost everywhere a shortage of primary weather data, and, understandably, an empirical approach to the practical problems for which the data are needed. Nevertheless it is encouraging to find so much being attempted in unpleasant working conditions; and stimulating to realize that the meteorologist can do much in the future to help the development of arid areas by providing information, and ideas to interpret the information.

H.L.P.

*Essai sur l'hydrologie superficielle au Sahara.* By Jean Dubief. Algerian Colonisation Service, 1953. Pp. 457; 41 Figs., 35 tables, charts. N.p.

This survey is in three parts of very unequal length. Main meteorological interest is in the short first part where knowledge of rainfall is discussed at length in terms of means, frequencies and distributions in space and time; the ignorance of evaporation is briefly and ruefully deplored.

The very long second part surveys the drainage basins separately, giving details of incidence of rain and resultant run-off.

In the final short part there is a general discussion of the part played by soil factors in helping to retain a surprisingly large fraction of the limited rain that falls, so much so that the Sahara can maintain a network of centres of human activity in spite of being a desert.

H.L.P.

*Report of the Desert Locust Survey and Control, Oct.* 1950-*Dec.* 1952. East African High Commission Desert Locust Survey, Nairobi, 1953. Pp. 64; 4 plates, 2 maps.

This report gives the history of the war against the desert locust from the time of the formation (October 1950) of the Desert Locust Control Organization up to the end of 1952. The work of the two years has shown in particular that British East Africa can be protected by killing locusts beyond its borders, and although much of the report is not primarily of interest to meteorologists, this war history makes instructive reading as an example of applied science.

The total cost of control measures during the period is stated as approximately £2,250,000, met by contributions from Eritrea, Kenya, Kuwait, Somaliland Protectorate, Sudan, Tanganyika, Tripolitania, Uganda and the United Kingdom.

There are applications of meteorology in all the research, survey and control aspects of the locust problem. The main factors in the development of a locust plague and the strategy of control are migration of the locusts and their need for warm damp sand or soil in which to breed. Thus the response of the locust at various stages in its life history to various conditions of temperature and humidity requires investigation.

The study of ' synoptic charts ' of swarm movements has been carried on in collaboration with the various weather forecast offices serving the area, from Nairobi to Cairo and Karachi, and as a result of this study of reported movements of swarms Dr. Rainey has suggested that, in general, major swarm movements take place down wind, such movements bringing swarms to regions of convergence of air (and ultimately rainfall).

Dr. Rainey's hypothesis gives a ready explanation of how locusts arrive in the breeding areas when it rains, and is said to have begun to rationalize locust forecasting.

It may be mentioned that Dr. Rainey has developed his hypothesis somewhat farther than is indicated in the report, to an association of swarm distribution with the intertropical convergence zone (*Nature*, **168**, p. 1057, 22 Dec. 1951).

S.T.A.M.

# Obituary Notices

## Mr. H. H. HARDING

On 20 March 1953 the Society lost its senior Fellow by the death at his home at Frampton Cotterell, near Bristol, of Henry Herbert Harding, whose election took place as long ago as 18 January 1893. He had thus held Fellowship over a period of 60 years. This, though falling far short of the late Richard Inwards's 75½ years on the membership roll, is a record which not many other Fellows of the Society have matched. Mr. Harding's interest in the weather extended back to his boyhood. He started daily meteorological observations in 1883, at the age of 13, and maintained them with regularity until failing eyesight intervened soon after he had entered his ninth decade. He was widely known throughout the West Country both in his business capacity as a horticulturist and fruit grower and as a journalist. For more than half a century he was weather correspondent to the *Western Daily Press*. He also made numerous contributions to local newspapers; these included a service of special forecasts to the *Bristol Evening News*, which won him a considerable reputation for reliability and were continued until that journal ceased publication. Summaries of Mr. Harding's rainfall records, kept initially at Fishponds, Bristol, and later at Frampton Cotterell, have appeared in the annual volumes of *British Rainfall* since 1893; he was also responsible for the climatological data relating to the latter place which were given in the *Monthly Weather Report* from January 1943 until early this year.

E.L.H.

## Mr. H. A. P. JENSEN, B.A.

Hereward Anthony Jensen, a Fellow of the Society since 17 January 1945, died at the age of 32 from the effects of an accident sustained while journeying homeward by railway after a meeting of the Royal Geographical Society on 11 May 1953. Mr. Jensen received his early education at Oldershaw Grammar School, Wallasey, and graduated with first-class honours in geography at the University of Liverpool, where he was awarded the Humboldt Prize. From 1942 to 1944 he served as temporary civilian officer with the Intelligence Division of the Naval Staff at the Scott Polar Research Institute, Cambridge, contributing while there to a number of the Admiralty geographical handbooks. In 1944 he was appointed to an assistant lectureship in geography at the University of Manchester. After four years (1946-1950) given to research for the Joint Intelligence Bureau, Ministry of Defence, he proceeded to a post as physiographer and naturalist on the staff of the Nature Conservancy, London. In 1952 he was one of the delegates from that body to the International Geographical Congress held at Washington, U.S.A. By then he had become known for his efficiency in both research and administration. Coastal studies were latterly Jensen's main interest. He had planned an ambitious programme of inquiry into the disastrous floods along the eastern seaboard of England early in February 1953. Although he did not live to complete this work he was able to present some preliminary results of his investigations in two important articles entitled ' Tidal inundations past and present ' which he wrote for the March and April numbers of *Weather*. These stand as a monument to his promise of high achievement.

E.L.H.

## Corrigendum to July 1953 issue

E. E. Adderley, p. 388—

The final formula should read,

$$N_2 = \frac{n_3 - PN_2}{1 - P}$$

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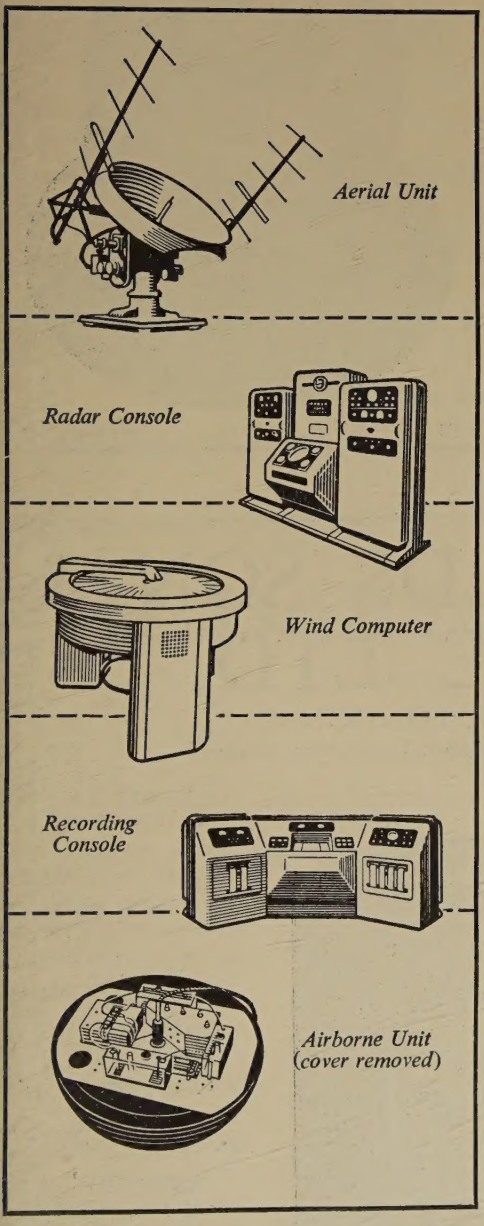


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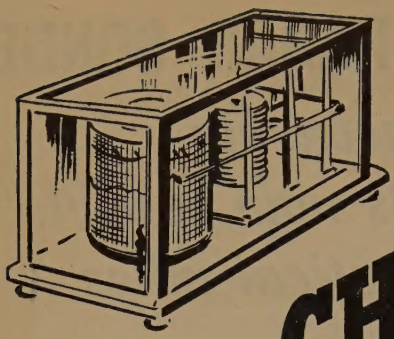

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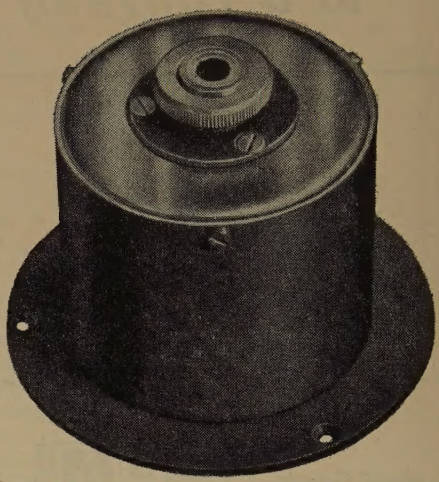

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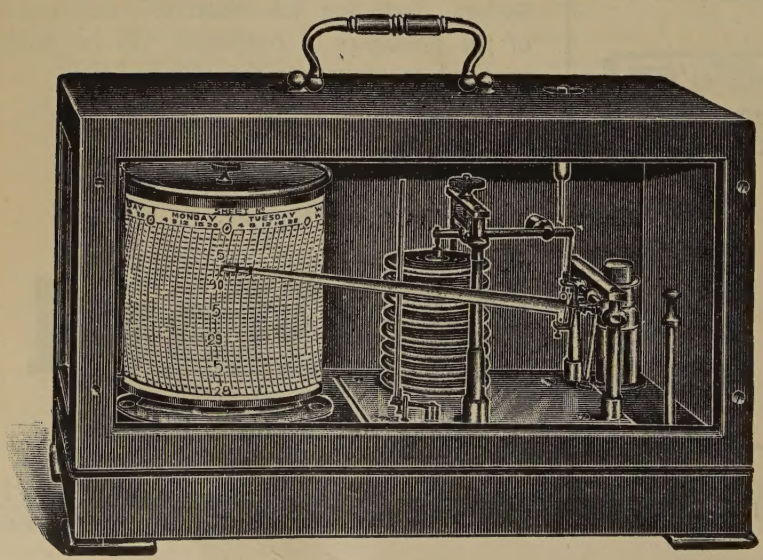

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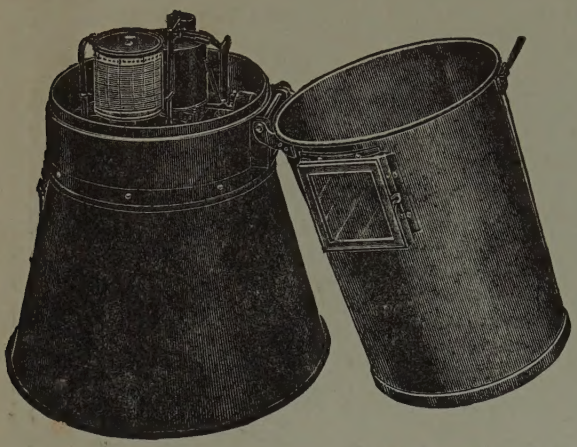

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